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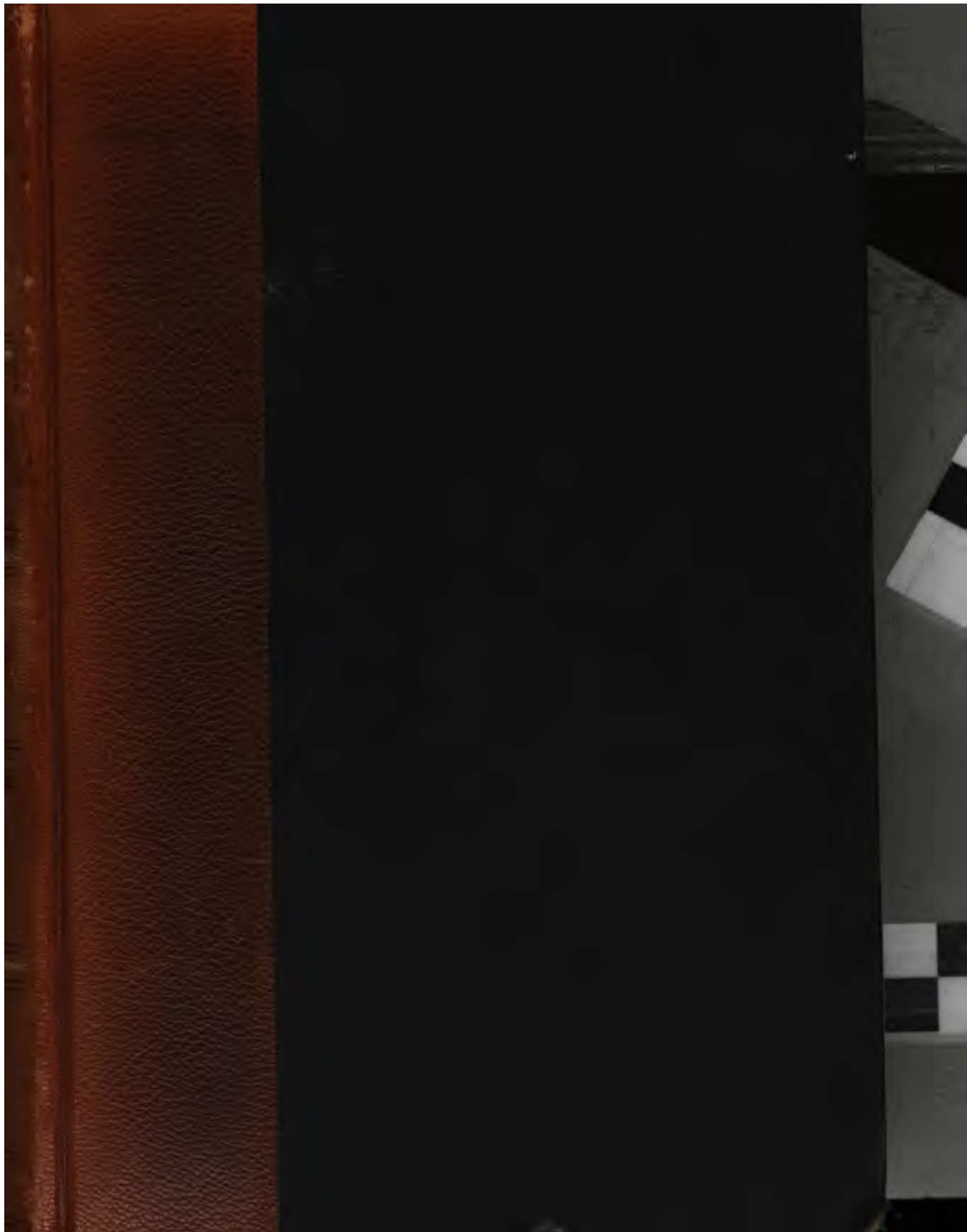
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# THE AMERICAN JOURNAL OF PSYCHOLOGY

EDITED BY  
EDWARD BRADFORD TITCHENER

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## THE AMERICAN JOURNAL OF PSYCHOLOGY

Thirty-four years ago, in November, 1887, I falteringly issued the first number of THE AMERICAN JOURNAL OF PSYCHOLOGY. There was then no other periodical on this subject in the English language except *Mind*, which was established in 1876 by Croom Robertson, who had told me the interesting story of its inception in a way that filled me with emulation. *Mind* represented advanced British thought, although it had printed not a few of my own articles and those of my students before we had an organ of our own. Both its spirit and its field were very different from the journal I intended, which I wanted to have, first of all, an exponent of experimental laboratory psychology. As, however, there was then no such laboratory in the country save my own at the Hopkins, the supply of material for the six hundred pages per annum that our prospectus had announced was very meager. This was fortunate for the *Journal*; for we had to make a virtue of necessity and include non-experimental articles over a wide range, drawing upon the fields of philosophy, logic, aesthetics, and religious, educational, pathological and comparative psychology. Even thus the supply of acceptable materials was scanty. Hence a very large part of the early volumes was devoted to book-reviews, excluding none of these fields. The vast majority of this small-type material was written by my-



self, and this required diligent and almost incessant reading and epitomizing.

Moreover, the *Journal* was a personal enterprise, and for years it was a heavy drain upon my own purse. My hopes and expectations had been extravagant, as I realized to my grief. Subscribers were few, and while there were some cordial expressions of appreciation, *e.g.* from James and Ladd, there was plenty of criticism from conservative instructors in this general field like Dr. McCosh, who felt that the position he had taken in his psychology, that mind and body were utterly disparate and incommensurable, was challenged in our prospectus. The negative attitude of the *Journal* toward telepathy and the English psychic researchers lost us not a few subscribers and alienated a patron who had made a generous and unsolicited contribution—the only one the *Journal* ever had—on the assumption, as it afterwards appeared, that the *Journal* would favor this cult. Thus the second year saw an actual reduction of our subscription list, and only with the third volume did we find our own; and most of the subscribers to that volume are with us yet. Thus for years I was editor, publisher, chief contributor, and sole financial support of the *Journal*, in which at its darkest period I had sunk over eight thousand dollars of my own meager savings, which I saw little prospect of ever regaining. But I was committed to the enterprise and must “carry on.” And in this I was encouraged, first by the spirit of Hopkins, and later by the generosity of Jonas G. Clark, our Founder, who employed Dr. Sanford to edit the *Journal* during 1888–89, when I was abroad. With the growth of departments of psychology in the country, and after the organization of The American Psychological Association at Clark in 1890, the fortunes of the *Journal* began to improve, and they have done so slowly and surely ever since.

The chief good fortune in the history of the *Journal* was the advent of E. B. Titchener of Cornell as one of the associate editors in 1895, and gradually the contributions from him, his laboratory, and his pupils have taken the predominant place in its pages. In all the vicissitudes of the *Journal* he

has stood loyally by, and but for his coöperation the *Journal* would have had a far different and less useful and honorable career. New journals have appeared, several of which at first seemed likely to interfere with our circulation, but the record shows that that of the *American Journal* has steadily increased.

In transferring the control of the *Journal*, as I do with this number, to its new editor, I feel, as is only natural, that to part with the child that has occupied so much of my care for so long marks a break which is less only than retiring from Clark, which is only slightly younger than it. Professor Titchener is, however, my natural successor and heir, the only one to whom it ought to go, and under whose management I am rejoiced to feel that the *Journal* will enter upon a new era. I believe that he will not only realize many hopes of my own that I have not been able to bring to fulfillment, but will make the *Journal* a leader and a light as it has never been before. I have asked and received no assurances from him regarding the fate of any of "my policies," so that he is absolutely free to do and make what he will of the *Journal*. But I know something of his ideals, and they have not only my most enthusiastic endorsement, but I bespeak for him all the goodwill and support both from contributors and subscribers that the *Journal* has previously enjoyed, and am confident that under his guidance it will enter upon a larger career of service than it has hitherto known. On the day Clark University opened, the University of Berlin cabled us its *Vivat, Crescat, Floreat*. This is my message to my successor.

G. STANLEY HALL.

The most welcome announcement that I can make to readers of the *Journal* is, I am sure, this: that Dr. Hall does not wholly sever his connection with us, but on the contrary promises us his active sympathy and support. I wish we could make him realize how much this means to us!

As for policies, I did not know that he professed any,—unless, indeed, a wide interpretation of the word 'psychology,' a generous catholicity of attitude toward every movement that

touched psychology and might help to advance it, an eager and yet discriminating enthusiasm for new psychological trends, and a steady adherence to the ideals and standards of scientific work, may be said to constitute policies. I shall, at all events, try in these regards to follow as closely as I may in Dr. Hall's footsteps. We cannot improve upon those early volumes of which he speaks so modestly; but I hope, with the valued aid of my co-workers, to make the future *Journal* worthy of the past.

E. B. TITCHENER.

EARLY PSYCHOLOGICAL THEORIES  
OF  
HERBERT SPENCER

---

By GEORGE BION DENTON  
Northwestern University

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The mature views of Herbert Spencer upon the science of psychology were conceived somewhat under the influence of British philosophy and more under the influence of Biological Evolution. But earlier, he held views representing, in the main, a quite different development. Though the British philosophy was already present, the evolutionary thinking was replaced by a heterodox phrenology. These earlier views might, perhaps, be disregarded—as Spencer himself disregarded them in his *Principles of Psychology*—if it were not that they “adumbrated” (as he might have said) theories which have been developed—and supposedly have been originated—within recent years. Presenting crude notions of Fatigue and of Attention, they constitute a real, though isolated, chapter in the history of modern psychology.

The later psychological views of Spencer were, of course, developed at length in his *Principles of Psychology*, published in 1855; his earlier views, in their completest form, furnished the basis of psychological theory for the essay *The Philosophy of Style*. It may seem remarkable that these two works, representing radically different points of view, should have been published within three years of each other; and the difficulty is increased by the fact that Spencer's letters of the spring of 1852 show that by that time he had already planned his *Psychology* and was at work upon it.<sup>1</sup> The difficulty is removed by the hypothesis that *The Philosophy of Style*, being a revision of an essay, *Force of Expression*, written

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<sup>1</sup> *Autobiography*, I, 452-454.

about 1844,<sup>2</sup> differed very little in substance from the earlier essay, except in an added paragraph at the close designed to give the essay an evolutionary turn.<sup>3</sup>

At the age of eleven or twelve, Spencer became a believer in phrenology, and continued such for many years.<sup>4</sup> In time, however, his views ceased to be in strict accord with the leaders of phrenological theory,<sup>5</sup> and the three articles<sup>6</sup> which Spencer published in the *Zoist*, a phrenological journal, in 1843 and 1844, were all heretical. Early in 1846 Spencer devised a Cephalograph, an instrument for making accurate measurements of skulls. Before executing a workable instrument of this design, he became entirely skeptical of phrenology,<sup>7</sup> and his interests turned to other schools of psychology. The *Principles of Psychology*, when it appeared in 1855, was, on the whole, opposed rather than inclined to phrenology, and a passage of several paragraphs<sup>8</sup> was devoted to a judicious criticism of phrenology as a system and to a denial of some of its leading positions.

In *The Philosophy of Style*, Spencer did not attempt to set forth a complete system of psychology, phrenological or otherwise, but attempted to expound only such principles of mental operation as would explain the effective use of language in discourse. Three features of this psychology are especially interesting—the implied theories of the Faculties, of Fatigue, and of Attention.

### THE FACULTIES

By such expressions as "every faculty"<sup>9</sup> and "group of faculties,"<sup>10</sup> in *The Philosophy of Style*, Spencer implied that the faculties were numerous. He named several, such as the "faculty of reverence"<sup>11</sup> and the faculty of "appropriation."<sup>12</sup>

These faculties, he apparently regarded, not merely as

<sup>2</sup> *Ibid.*, I, 258; 468-469; *Life and Letters*, I, 86.

<sup>3</sup> The proof of this hypothesis the writer has undertaken in a separate study.

<sup>4</sup> *Autobiography*, I, 227-231.

<sup>5</sup> *Ibid.*, I, 261, 282; *Life and Letters*, II, 310.

<sup>6</sup> "A New View of the Functions of Imitation and Benevolence," *Zoist*, I (1843); "On the Situation of the Organ of Amativeness," and "A Theory concerning the Organ of Wonder," *Zoist*, II (1844).

<sup>7</sup> *Autobiography*, I, 634.

<sup>8</sup> Part IV, ch. 8, 606-611.

<sup>9</sup> *Westminster Review*, 58 (1852), 446.

<sup>10</sup> *Ibid.*, 455.

<sup>11</sup> *Ibid.*, 456.

<sup>12</sup> *Ibid.*, 456.

subjective entities, modes of operation of the mind, but as physiological organs. His theory of fatigue, which will be dealt with in the next section, is evidence of their physiological nature.

The faculties were roughly classified. ". . . In the reflective faculties," he wrote, "in the imagination, in the perceptions of the beautiful, the ludicrous, the sublime, in the sentiments, the instincts, in all the mental powers, however we may classify them . . . ." <sup>13</sup> Apparently this was not meant for a systematic classification, but it seems possible to distinguish three or four groups, such as "reflective faculties," "sentiments," "instincts," and possibly "perceptions." Spencer frequently spoke of "perceptive faculties," <sup>14</sup> at other places in the essay.

Nothing comparable to these minute and numerous faculties suggested in *The Philosophy of Style* appeared in the *Psychology*. In the latter work, the structural elements of mind were the same as those of the British sensationists. The aspects of Intelligence which Spencer treated were Reflex Action, Instinct, Memory, Reason, the Feelings, the Will, <sup>15</sup> and none of these (as he carefully pointed out) is distinct from another. <sup>16</sup> In the *Psychology* his chief criticism of phrenology was that it made too sharp the demarcation of the faculties. <sup>17</sup> On the contrary, in *The Philosophy of Style* it is the distinctness and the unalterable character of the individual faculty that explains the effect of such devices as Antithesis, Climax, and Anticlimax. "The opposition of two thoughts that are the reverse of each other in some prominent trait insures an impressive effect; and does this by giving a momentary relaxation to the faculties addressed." <sup>18</sup>

This feature of Spencer's early psychology was equally unlike anything held by the British philosophers. The latter, though differing considerably among themselves in regard to the number of faculties, usually conceived a three to five-fold division, comprising such general faculties as Intellect, Emotion, Will. Moreover, none of the British philosophers, not even Hartley, associated the faculties with distinct organs of the brain.

When compared with the faculties as conceived by phrenologists, however, the faculties as represented in *The*

<sup>13</sup> *Ibid.*, 455.

<sup>14</sup> *Ibid.*, 454.

<sup>15</sup> *Principles of Psychology*, Part IV, chs. IV-IX.

<sup>16</sup> *Ibid.*, 584.

<sup>17</sup> *Ibid.*, 609.

<sup>18</sup> *Westminster Review*, 58 (1852), 456.



*Philosophy of Style* were (it is hardly necessary to show) very similar to those of phrenology; and such phrenological names as "reverence," "ideality," and "approbativeness,"<sup>19</sup> for example, corresponded to Spencer's "reverence," "beauty," and "approbation."<sup>20</sup> The phrenological faculties, too, like Spencer's, were distinct, physical, and anatomically localized.<sup>21</sup> Similarity in the classification of the faculties may also be noticed, for Spurzheim divided them into Intellectual and Affective, with subdivisions of Reflective and Perceptive under the first head, and Sentiments and Propensities under the second.<sup>22</sup>

#### FATIGUE

Spencer's theory of fatigue, stated most definitely in the latter part of the essay, was implied throughout. In the third paragraph, explaining the general principle of economy of attention in the use of language, Spencer wrote: "A reader or listener has at each moment but a limited amount of mental power available. To recognize and interpret the symbols presented to him requires part of this power: to arrange and combine the images suggested requires a further part; and only that part which remains can be used for the realization of the thought conveyed."

Later, a more explicit statement was made: "Without going at length into so wide a topic as the exercise of faculties and its reactive effects, it will be sufficient here to call to mind that every faculty (when in a state of normal activity) is most capable at the outset; and that the change in its condition, which ends in what we term exhaustion, begins simultaneously with its exercise. This generalization, with which we are all familiar in our bodily experiences, and which our daily language recognizes as true of the mind as a whole, is equally true of each mental power, from the simplest of the senses to the most complex of the sentiments. If we hold a flower to the nose for long, we become insensible to its scent. We say of a very brilliant flash of lightning that it blinds us; which means that our eyes have for a time lost their ability to appreciate light. After eating a quantity of honey, we are apt to think our tea is without sugar. The phrase 'a deafening roar,' implies that men find a very loud sound temporarily incapacitates them from hearing faint ones. Now, the truth

<sup>19</sup> Spurzheim, *Phrenology*, 225-228; 243-245; 212-214.

<sup>20</sup> *Westminster Review*, 58 (1852), 456.

<sup>21</sup> Spurzheim, *Phrenology*, 90-98.

<sup>22</sup> *Ibid.*, 149-151.

at once recognized in these, its extreme manifestations, may be traced throughout; and it may be shown that . . . in all the mental powers . . . action exhausts; and that in proportion as the action is violent, the subsequent prostration is great."<sup>23</sup>

Discussions of fatigue as a factor in mental phenomena are common enough in the psychologies of today, but in 1852 and before one would have looked for them in vain in the works of British psychologists and philosophers. Some phases of Spencer's own biological view of psychology set forth in the *Psychology* would have accommodated his earlier theory of fatigue, but nothing of the sort appeared in that work.

Phrenologists, however, made considerable use of the idea of fatigue. "The faculties of animal life," wrote Spurzheim, "cannot act incessantly, they require repose. Study of the same subject, too long protracted, causes fatigue; by changing this we may still continue our labors. Now if the brain were a single organ, that performed all the functions of the mind, why should it not be still further fatigued by this new species of action? Although our eyes be fatigued by looking at pictures, we can still listen to music, because there is a particular organ for each of these sorts of impressions."<sup>24</sup> "As during watching the same organ is not always active, but reposes at intervals; so, during sleep, all the organs do not sink into inactivity together, but a particular one continues its function, and then the peculiar state called dreaming supervenes. . . . Every corporeal organ being fatigued takes rest, and this state of rest is sleep; but single, or even several organs, may be active while the others repose."<sup>25</sup>

#### ATTENTION

Attention is the most interesting of Spencer's early psychological conceptions. In the first part of the essay (consisting of fifty paragraphs) attention was the most comprehensive power of intellect, and its economy was represented as the great desideratum in the use of language.

In several respects, attention, as Spencer conceived it, had features in common with attention as it is generally regarded by psychologists today.

On the subjective side, the result of attention upon the idea seems to be to increase the effectiveness of the idea.

<sup>23</sup> *Westminster Review*, 58 (1852), 437.

<sup>24</sup> Spurzheim, *Phrenology*, 95.

<sup>25</sup> *Ibid.*, 96.

Spencer used commonly the terms "vivid,"<sup>26</sup> "forcible,"<sup>27</sup> and "effective"<sup>28</sup> to denote the influence of attention upon the content of mind. Other subjective features, such as the "selective" function of attention and its effect upon retention in memory, scarcely arose for consideration.

The physiological character of Spencer's view of attention is most noteworthy. Attention is apparently a reservoir of physical energy at the service of mental life. Every mental act of any sort requires attention; it uses up energy. The energy of attention is associated with the activity of the whole mind, or rather with every part of it. It is a fixed amount—"A reader or listener has at each moment but a limited amount of mental power available"<sup>29</sup>—and it may be temporarily drained to varying degrees of fatigue or exhaustion. The attention is "absorbed"<sup>30</sup> or "frittered away,"<sup>31</sup> or there is a "strain upon the attention."<sup>32</sup> Moreover, it may be drained through many channels; that is, it is applicable to the needs of any faculty. Finally, attention reinforces the activity of specialized portions of the brain; and apparently this physiological reinforcement corresponds to the mental effectiveness produced by attention. Attention may be taxed by severe demands upon one faculty or by multitudinous demands upon many faculties simultaneously. In one place Spencer began, ". . . If some subtlety in the argument absorb the attention—if every faculty be strained in endeavoring to catch the speaker's or writer's drift—"<sup>33</sup> Very clearly "straining every faculty" is the same as "absorbing the attention;" and indicates as clearly as may be in brief the relation of attention and faculties, explained above.

Dominant as attention was in *The Philosophy of Style*, it did not appear again as a psychological entity of importance in Spencer's works. In *The Philosophy of Style*, the word "attention" appeared seventeen times in fifty paragraphs.<sup>34</sup> In *Principles of Psychology*, the word appeared twenty times in six hundred and twenty pages. Six<sup>35</sup> of these latter

<sup>26</sup> *Westminster Review*, 58 (1852), 439.

<sup>27</sup> *Ibid.*, 440.

<sup>28</sup> *Ibid.*, 451.

<sup>29</sup> *Ibid.*, 437.

<sup>30</sup> *Ibid.*, 438.

<sup>31</sup> *Ibid.*, 451.

<sup>32</sup> *Ibid.*, 454.

<sup>33</sup> *Ibid.*, 446.

<sup>34</sup> *Ibid.*, 436, 437 (three times), 438 (twice), 439, 440, 443, 446, 449 (twice), 451, 454 (four times).

<sup>35</sup> *Principles of Psychology*, 122, 127, 209, 363, 449, 616.

instances had no psychological significance, and none of the others<sup>36</sup> indicated anything like a theory of attention.

Whence could the suggestion for his theory of attention have come? Modern writers upon psychology assert that the British thinkers did not study attention, as it is conceived today. "With some notable exceptions (Wolff, Kant, and James Mill) the attention was greatly neglected until more modern times, notably by the English empiricists. . . . It was considered an unanalysable attribute of the soul, and direct evidence of the independent activity of the mental principle (Hamilton, Carpenter, McCosh)."<sup>37</sup> Professor James wrote: "Strange to say so patent a fact as the perpetual presence of selective attention has received hardly any notice from psychologists of the English empiricist school. The Germans have explicitly treated of it, either as a faculty or as a resultant, but in the pages of such writers as Locke, Hume, Hartley, Mill, and Spencer the word hardly occurs, or if it does, it is parenthetically and as if by inadvertence."<sup>38</sup>

Such writers, British and Continental, as may have treated of attention were probably all unknown to Spencer. He was never well read in philosophy, and the little that he knew by 1852 was mostly second hand. His own statements<sup>39</sup> in regard to his philosophical reading would make certain that no suggestion for his conception of attention could have been derived from the philosophers.

Attention was certainly not a fundamental and characteristic conception of phrenology; yet it was a conception that the phrenologists were constantly meeting on the Continent. Such conceptions as Memory, Association, and Will, Spurzheim did not directly attack, but reinterpreted in harmony with phrenology. Similarly, with attention. He wrote: "The word *attention* denotes no more than the active state of any intellectual faculty; or, in other terms, attention is the effect of the intellectual faculties, acting either from their proper force, or from being excited by external impressions, or by one or several affective faculties. Hence there are as many species of attention as fundamental faculties of the mind. . . . It is, indeed, absurd to expect success in an art or science, when the individual power on which its comprehen-

<sup>36</sup> *Ibid.*, 194, 195, 235, 281, 282, 299, 315, 477, 495, 503, 546.

<sup>37</sup> Baldwin, *Dictionary of Philosophy and Psychology*, article on "Attention."

<sup>38</sup> James, *Principles of Psychology*, I, 402.

<sup>39</sup> *Life and Letters*, I, 145-147.

sion depends is inactive. Again, the more active the power is, the more it is attentive."<sup>40</sup>

Coupled with the phrenological view of physiological exhaustion of the faculties, this doctrine would hold that attention is the state of greatest physiological activity of an "organ" of the brain; and the greater the task of the organs or faculties, the greater the exhaustion of the attention. There was much in this phrenological view that is like Spencer's theory in *The Philosophy of Style*.

The conception of attention which Spurzheim was forced grudgingly to admit existed as a mental phenomenon of secondary importance, Spencer, the phrenological heretic, might well have welcomed as a fundamental organ of mind. Certain features of Spencer's article, "A Theory concerning the Organ of Wonder," published in the *Zoist* in 1844, point to such a view.

Spencer there proposed that what was commonly regarded by phrenologists as the Organ of Wonder was in truth an organ whose main function is to recall impressions once received.<sup>41</sup> This theory was proposed by Spencer as supplementary to an article of the previous year, entitled "*Imitation and Benevolence*."<sup>42</sup> In that article he reinterpreted the Organ of Imitation into an organ whose function was to excite sympathetic states of mind, while the Organ of Benevolence became "the grand centre of sensation, and is excited by the affections of all the other organs."<sup>43</sup> The names for the three organs, Wonder, Imitation, and Benevolence, became then Reviviscence, Sympathy, and Sensitiveness, respectively. The whole theory was summed up as follows, in the later article: "It was maintained that it is the primary office of the organ entitled Imitation, to excite in the mind of one being the feelings exhibited in another, and it is the aim of the present essay to show that the true duty of the adjoining organ, hitherto called Wonder, is the revival of intellectual perceptions. It is the object of both to bring certain other faculties into activity. By the one, feelings are recalled; by the other, impressions."<sup>44</sup>

The next to the last sentence is to be noted: Reviviscence acts as a reenforcement of the intellectual faculties. In this respect, although applied to the service of memory only, it is

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<sup>40</sup> *Phrenology*, 380.

<sup>41</sup> *Zoist*, II (1844), 316-325.

<sup>42</sup> *Zoist*, I (1843).

<sup>43</sup> *Ibid.*, 377.

<sup>44</sup> *Zoist*, II (1844), 322.

like attention in *The Philosophy of Style*, and might have been adapted to the needs of that essay.

Interesting in this connection is the fact that Dr. Bernard Hollander has identified Spencer's Organ of Wonder with the modern conception of attention. Dr. Hollander pointed out that Spencer, by assigning to that organ the function he did assign, anticipated the work of Professor Ferrier in regard to localization of functions in the brain.<sup>45</sup> And this conclusion Dr. Hollander arrived at, apparently, without knowledge of Spencer's use of attention in *The Philosophy of Style*.

Dr. Hollander quoted Ferrier in regard to the function of "area 12," which corresponds to the Organ of Wonder: ". . . the excitation of which causes the eyes to open widely, the pupils to dilate, with movements of the eyeballs and head. It gives the appearance of attention, and the movements indicated are essential to the revivification of ideas."<sup>46</sup>

If, as Dr. Hollander believed, Spencer's Organ of Reviviscence anticipated the modern notion of the function of attention and its location in the brain, then it is very likely indeed that, in *The Philosophy of Style*, attention is merely a development and new application of the phrenological theory of reviviscence.

That Spencer should have changed the name of attention in the essay on style is easily understood. For one thing, the application to memory was not largely involved in the later essay. For another, Spencer admitted that the name "reviviscence" was awkward.<sup>47</sup> Finally, it is probable that in writing for *Westminster Review* (in which the essay on style appeared), Spencer was not desirous of suggesting totally unfamiliar terms, or phrenological terms which might call his psychology in question. He probably adopted the name "attention" because it was used by phrenologists in somewhat the same sense as he employed it, and because he found it occasionally in works on rhetoric<sup>48</sup> from which he borrowed considerably in writing *The Philosophy of Style*.

<sup>45</sup> *Westminster Review*, 139 (1893), 142-154.

<sup>46</sup> *Ibid.*, 148.

<sup>47</sup> *Zoist*, II (1844), 320.

<sup>48</sup> Spencer quoted from Blair the statement that "long sentences fatigue the reader's attention." See also, Denton, "Herbert Spencer and the Rhetoricians," in *Publications of the Modern Language Association of America*, XXXIV (1919), 89-111.



## THE COMPARATIVE INFLUENCE OF MAJORITY AND EXPERT OPINION

By HENRY T. MOORE, Dartmouth College

The literature on Social Psychology contains numerous references to the influence of the group on the opinions of the individual. This one point has been made the subject of practically an entire volume by Trotter,<sup>1</sup> who refers to group opinion somewhat picturesquely as the voice of the herd. He represents this voice as coming with such a weight of authority that even the most eccentric individual feels compelled to seek some form of herd support for his opinions, and is completely at a loss when no such support is anywhere to be had.

The general fact is beyond dispute, but those who would like to see Social Psychology multiply its experimental findings are tempted to ask more specifically just how great this influence may be expected to be in any given situation. Can we hope to measure it? And if so, how does it compare with other influences that are likely to operate in determining an individual's social attitudes?

The group experiment here reported attempts a beginning at answering these questions for three types of situation,—namely, speech, morals, and music. The method is somewhat similar to one used by Bridges<sup>2</sup> in a study of decision types reported in 1914. In general it consists of measuring a suggestive influence in terms of the number of reversals of judgment occasioned by it, as compared with the number that might have been expected by chance. The first problem was therefore to find out what was the chance of reversal of judgment in regard to each of the three kinds of material used. Ninety-five subjects were given eighteen paired comparisons for each of the three types of situation. The instructions for the linguistic judgments were that the subjects check the more offensive one of each pair of expressions. Examples of the expressions compared are: "Everybody loves

<sup>1</sup> Trotter, W., *The Instincts of the Herd in Peace and War*, 1916.

<sup>2</sup> Bridges, J. W., *An Experimental Study of Decision Types and their Mental Correlates*, *Psych. Rev. Mon. Sup.*, 1914, Vol. XVII, No. 1.

their mother." "She sort of avoided him." "The party that wrote that was a scholar." "He never studies nights." The ethical judgments involved the checking of the more offensive of two traits of character in each of the eighteen pairs. Examples of the traits compared are:—disloyalty to friends; willingness to get rich by questionable financial methods; cheating on an examination; willingness to overlook a business error favorable to oneself. The musical judgments involved an expression of preference for one of two resolutions of the dominant seventh chord, played on a reed organ. Eighteen paired resolutions were played, and the preferences recorded after each.

Two days later the same three series were repeated exactly as given before, and without the introduction of any special suggestive influence to alter the original judgments. Each subject was now scored on the basis of his percentage of reversals, and the mean of the ninety-five individual scores was taken as the chance of reversal for judgments concerned with that particular type of material. The average score thus recorded as representing the chance of reversal for linguistic judgments was 13.5 per cent with .55 P. E. of the mean; for moral judgments 10.3 per cent with P. E. of .50; for musical judgments 25.1 per cent, with P. E. of .84. These results are indicated graphically in the upper accompanying chart.

As a partial check on the above figures each of the three series of judgments was tried on a different group. The subjects in the check experiment were on an average about a year older than those in the original experiment, which probably accounts for their slightly lower per cent of reversals. Forty-three subjects gave 11.4 per cent chance of reversal in linguistic judgments; 62 subjects gave 9.4 per cent reversals in ethical judgments; and 49 subjects gave 22.6 per cent reversals in musical judgments.

An interval of two and a half months was allowed between the experiment without suggestion and that in which suggestion was used. This seemed ample time to render negligible any memory effects from the preceding judgments. The experiment was now repeated as before, except for the addition of the suggestive influences. A new set of original judgments was taken, and after a two day interval the subjects were given the same series again, this time with the statement of what had been the majority preference for each pair. Great care was taken to convince them that these statements were being truthfully made, and the influence of suspicion was certainly

great. Each subject was now scored on the basis of the per cent of opportunities he had accepted to reverse his judgment so as to agree with majority opinion. The results indicated in the three left columns of the upper chart. The average of such reversals in linguistic judgments was 50.1 per cent, with P. E. of 1.63; in ethical judgments 50.1 per cent, with P. E. of 1.69; in musical judgments 48.2 per cent, with P. E. of 1.52.

70 days later still the same comparisons were repeated, but this time each judgment was preceded by a statement of opinion of an expert in each field. These statements in most cases coincided with what had been given as majority opinion previously, but as often as not they were at variance with it. From these last records each individual was scored on the basis of the per cent of opportunities he had accepted reversing his original judgment favorably to the statement of the expert. The results, which are indicated in the three right columns of the upper chart, show an average of 48.2 per cent reversals of language judgments, with P. E. of 1.4; 50.1 per cent reversals of ethical judgments, with P. E. of 1.69; and 46.2 per cent reversals in musical judgments, with P. E. of 1.52.

Now we take as our unit of measurement the per cent predicted as the chance of reversal, we find, as indicated on the lower chart, that the probability of reversing favorably the majority in matters of speech and morals is approximately five times chance; whereas in matters of musical feeling the probability is only about twice chance. By majority is meant here of course only the special type of majority produced in the experiment, but if generalization is permissible on the basis of the evidence available, we may venture the judgment that a man is two and a half times as individualistic in his musical likes and dislikes as in his moral and linguistic preferences. Similarly we may conclude that expert and majority opinion hold about equal sway over the individual in matters of morals and music, but that the chances are about ten to one in favor of majority prestige in matters pertaining to speech. To take a concrete case, the struggle between the 'cant' pronunciation which accents the word 'cantonment' on the first syllable, and the 'army' pronunciation, which puts it on the second syllable, is likely to end with the doom of the expert, whereas classical music has at least an even chance in its struggle with the popularity of the jazz band. The type of experiment here described can, on account of the great range of individual variations involved, be valid

only if applied to a large number of cases; and inasmuch as each particular experiment measures only a very particular type of suggestibility, any generalization from a single experiment will always be questionable. But it is believed that an extension of the method to cover a large number of typical cases in which such social influences as personal prestige, fashion, orthodoxy, etc., play a part, would give material which would be valid for general purposes. Whether the shrunken prestige of a defeated political candidate or of an abdicated emperor follows any accurately describable laws, one could scarcely venture to say; but it is sufficiently obvious that until so-called social laws rest on more than the personal observations of individual writers, we shall have a great excess of laws, and only a minimum of confidence in applying them.<sup>3</sup>

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<sup>3</sup> Read at the Annual Meeting of the American Psychological Association, Cambridge, Mass., Dec. 31, 1919.

## THE NUMBER FORMS OF A BLIND SUBJECT

By RAYMOND H. WHEELER and THOMAS D. CUTSFORTH

Number forms in the adventitious blind are sufficiently rare to warrant the description of a case. The reagent whose forms are here described is T. D. C., one of the authors of this article and a former major student in psychology at the University of Oregon. On March 9, 1917, detailed descriptions, in writing, of these forms were obtained and filed away. On August 15, 1919 the reagent was unexpectedly asked to describe these forms again. These latter descriptions were then compared in minute detail with the earlier descriptions.

The reagent has a simple digit form for the numbers 1—10, a tens form for the numbers 10—100, a hundreds form for the numbers 100—1,000, a thousands form and so on into the millions. He also has a week form, an alphabet form, a month form and a century or date form. He also has numerous varieties of synaesthesia.

The lower right hand figure in the accompanying cut is the reagent's form for the numbers 1—10. Each rectangular section is sharply differentiated from its neighbor by means of hue and brightness qualities which stand for separate numbers. The numbers themselves are never visualized and when the subject uses this form he translates the colors into numbers vocal-motor fashion. The form appears in space in front of the reagent's face at a distance of about one meter. The entire form is about fourteen centimeters long and two centimeters in width. The following are the colors for the numbers 1—9, taken verbatim from introspections.

On March 9, 1917	On August 15, 1919
1. white	whitish, nothing but brightness
2. dull grey, tinged with yellow, like old weather beaten boards	grey, slightly yellow, like old weathered straw
3. like 2 but more reddish	light, faint reddish brown
4. like 3 but darker	peculiar brown, like fir boards

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|--|--|
| 5. muddy black                           | a dull black, like spilled ink                   |
| 6. white, like 1, identified by position | white, like ivory                                |
| 7. dark, muddy blue                      | bluish black, somewhat like graphite, but darker |
| 8. grey, tinged with yellow              | very poorly saturated yellow                     |
| 9. dull grey tinged with yellowish green | dark grey with greenish cast; a bottle green     |

The tens form consists of ten of the small digits forms placed end to end in a direction upward and away from the reagent toward the right, beginning at a point in front of his left shoulder. Each of the smaller unit forms represents figures from 1—9, 10—19, 20—29 etc. The extreme right end of the form bends more sharply away from the subject and upward less abruptly enabling him to visualize the entire form almost at once and to make use of it with less extended eye-movements. With the exception of the numbers from 10—19 each intermediate figure in the various ranges of tens is colored according to the final digit.

The hundreds form is a product of tens forms. The colors are determined in similar fashion. The thousands form is a product of hundreds forms and so on. In all of these latter forms the colors represent ranges and not individual numbers. For example in the hundreds form there are ten colors, representing the ten ranges of ten numbers each. In the thousands form there are ten colors representing ten ranges each of 100. When the reagent thinks of large numbers several forms are used and held in consciousness at the same time, the smallest form in the foreground and the larger forms localized back of the first one in order of their size. This can best be understood by reference to an analogy. Let each form be represented by a framed picture. The several forms in use at one time would then be represented by a series of framed pictures set up one behind the other, each one larger than the one in front of it and resting on a higher level. The individual forms retain their identity by their localization and by a colored halo which latter varies with each form. The halo would be represented by the frame of the picture. The halo or background for the hundreds form is black; for the thousands form it is silvery white; for the hundreds-thousands form it is reddish-brown and for the millions form it is yellow.

The following illustrates how the forms are used. When the reagent thinks of the number 3,591 a thousands form

appears in the background of a visualized field, with the appropriate section in the focus of attention, namely, the third section from the left hand end. Directly in front of this lies the hundreds form and with a "slide rule" effect the reagent's line of regard shifts to the fifth section of this latter form. A second shift of his line of regard brings the reagent's attention to the ninth section of the tens form which latter lies slightly below and in front of the hundreds form. A final shift carries his regard to the first section of the digits form which lies at the left hand end of the tens form, when thinking of large figures. The rapid use of number forms has been described several times in the literature. Diamandi's success as a lightning calculator was in part due to the use of a number form.<sup>1</sup> In the present case this fact is all the more remarkable for the reason that the numbers are not visualized. Colors are used as substitutes. Hennig,<sup>2</sup> Grüber,<sup>3</sup> and others have pointed out that in cases of synaesthesia colors have been used to represent numbers in performing mathematical operations. Galton<sup>4</sup> seemed to have been able to substitute odors for mathematical symbols in simple operations of adding and subtracting.

As an example of such a substitution of colors for numbers we give the following instance of counting in terms of colors. The authors were giving mental tests at the Oregon State School for the Blind. The reagent was in the act of counting the number of words which one of the blind pupils was giving within the interval of three minutes. He began by counting the words in vocal-motor fashion but in a short time vocal-motor imagery became confused and he was obliged to cease relying upon inner speech. In the meantime, however, each verbal image had been accompanied by its appropriate color, localized properly in the number forms. From this point on until the end of the three minutes the reagent, who was then acting as examiner, was able to keep track of the number of words which the pupil was reciting to him. The last color was then translated into the appropriate figure.

When the reagent is given large numbers to locate in these forms, eye-movements can be seen as he shifts his line of regard from one form to another or from one part of the

<sup>1</sup> See Philips, Genesis of Number Forms, *Amer. Jour. of Psychol.*, Vol. 8, 1897, 506-527.

<sup>2</sup> Entstehung und Bedeutung der Synopsien, *Zsch. f. Psychol.*, 1896, Vol. 10, 113-122.

<sup>3</sup> L'audition colorée, *Cong. inter. Psychol.*, Paris, 1889.

<sup>4</sup> Arithmetic by Smell, *Psychol. Rev.*, 1894, Vol. 1, 61-62.

same form to another part. The extent of eye-movement is proportional to the size of the forms which are being used. Voluntary or artificial checking of these eye-movements impedes the reagent in the use of the forms.

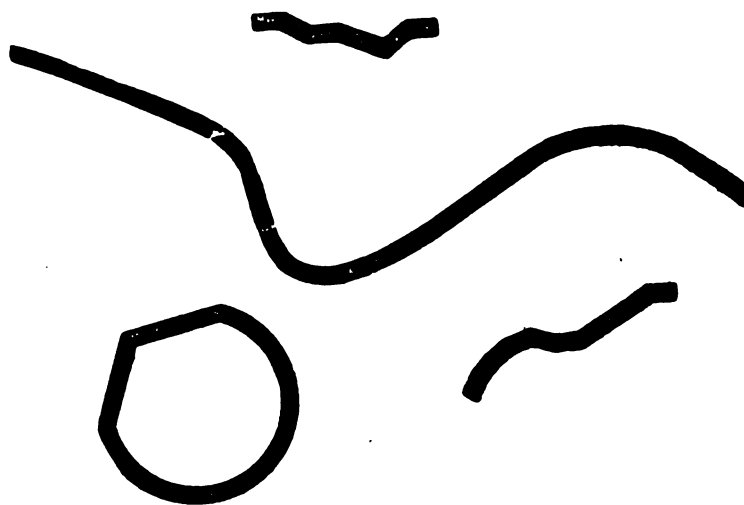
The upper figure of the cut is a miniature copy of the reagent's week form. Each day of the week is represented by a colored section in the form. Monday is dark, inky blue; Tuesday is pale brown; Wednesday is a very dark green; Thursday is a dark orange-red; Friday is a "russet" yellow; Saturday is a muddy, curdled black and Sunday is white.

The lower left hand figure is his month form. Again no letters or writing are seen in the form but the months are represented in colors. The flattened portion of the form corresponds to vacation periods of the school year. The subject is unable to recall the conditions under which the form originated.

The long figure in the cut represents the century or date form. It is about four centimeters wide and a meter long. It is visualized upon a globe-map of the world and upon this background appear dark, faintly colored areas as continents and light blue areas as oceans. The dates appear in color, not in figures or in writing. 1900 lies above the eastern coast of the United States; 1066 A. D. lies just above the British Isles; the dividing point between A. D. and B. C. in the region of the Mediterranean Sea and so on. The dates are colored according to their component digits. This form originated when the subject was first studying geography, and is used constantly as an aid in fixing and in recalling dates.

The figures in the cut are photographs of card-board models made by the reagent himself. In the actual forms there are no figures, the figures being inserted merely to explain the forms. The photograph was taken in March, 1917. In August 1919 the reagent made new models of all forms. During this interval of time none of these forms had changed except in minor degrees of saturation or in slants of some of the lines, and these differences may have been due to inaccuracies in making the model or in describing the colors. One exception must be noted, however. The century form originally slanted upward at its extreme right end. In the cut it is seen slanting downward. This change occurred during the time that the reagent was in college, and can be traced to the repeated use of this form while the reagent was studying Babylonian History. Since then the form has resumed its original upward slant.





**THE NUMBER FORMS OF A BLIND SUBJECT**

## SOME PROBLEMS IN REGARD TO ALIMENTARY SENSITIVITY

By IVY G. CAMPBELL

The observations around which the discussion in this article are centred were made under rather unfavorable experimental conditions and are offered not as conclusive evidence on disputed points but rather as suggesting problems and technique for future investigations.

The writer after several months of "stomach trouble" was put in March 1919 under the care of Dr. Einhorn of N. Y. City, whose very ingenious method of treatment gave her opportunity for the following study. The stomach was given a complete rest for two weeks during which time duodenal feedings were given. This was accomplished by direct feeding into the duodenum by means of a tube which passed from the mouth into the duodenum. This tube was not withdrawn during the two weeks period, but every day—from 6 a. m. to 8 p. m.—at two-hour intervals, from 240-300 c. c. of food (milk, raw egg, sugar of milk, and, toward the end of the treatment, butter) were forced through it into the duodenum. The apparatus which permitted this treatment is described by Dr. Einhorn as follows: "The duodenal pump consists of a small metal capsule (14 mm. long and 23 mm. in circum.) which is perforated and can be unscrewed. This communicates with a long, thin rubber tube (8 mm. in circum. and one metre long), and is marked at 40 (I. cardia), 50 (II. pylorus), 70 (III), and 80 cm. distance from the capsule. At its end is a tip, to which a syringe can be attached." (1926)

The greatest part of this paper will be devoted to a discussion of hunger, but some observations on and interpretations of appetite, fullness and emptiness, thermal sensitivity, will be given.

*Hunger.*—The experimental findings of Cannon and Washburn, later verified and extended by Carlson, Roring, and others, have convinced most readers that hunger is a sensation or sensational complex concomitant with the periodic, intermittent contractions of the empty or nearly empty

stomach. Carlson holds that each separate contraction is synchronous with a hunger pang, and that the intensity and duration of the hunger correspond pretty closely with the strength and the duration of the stomach contractions. He also found that his subjects locate the sensation of hunger in the stomach. His data obtained from observations on fasting subjects is of special interest in connection with the observations reported in this paper.

In the case where Carlson and one of his assistants underwent a five day starvation period they found that there was an increase rather than a decrease of the gastric tonus and the hunger contractions during the period (4:128); that the sensation of hunger was almost continuous after the first day of starvation, i.e. it did not wholly disappear during the intervals between the vigorous gastric contractions; and that during the first three days the hunger was greater than during the last two although the contractions were greater in the last two days than on the first three. (4:135) They also found that appetite was increased during the first few days and then decreased so that toward the last there seemed to be indifference toward food "despite the persistent hunger call of the stomach." (4:136) The discrepancy in the parallelism between the intensity of the gastric hunger contractions and the intensity of the subjective hunger sensations was due, Carlson is inclined to believe, to the depression of the Central Nervous System. (4:136) In the case of the man who under Carlson's observation completely fasted for fifteen days and for the subsequent eight days except for the daily injection of a quantity of cotton fibre it was found that the hunger contractions were continuous with practically normal rhythm and intensity but that the subjective sensations appeared to be somewhat weakened, tinged with an element of general epigastric distress or sick stomach. The appetite sense or desire for food was modified or obscured by a tendency to a persistent bad taste in the mouth, yet the dominant element in consciousness was reported to be the thought of food and eating. (5) In the treatment given the present writer the stomach was kept empty—except for one half-wine-glass dose of liquid medicine (bismuth and magnesium) given three times a day—for a period of fourteen days. Despite this continued emptiness, hunger—except in the few cases noted below—was not felt. This result seems at variance with those of Carlson and considered in connection with the manner of her treatment gives rise to a number of questions: (1) Are stomach contractions inhibited by duode-

nal feedings; (2) If not, is it then true that stomach contractions are the most important concomitant physiological factor in hunger; (3) Is it possible, as has been suggested, that intestinal contractions play a large, if not the principal part, in hunger and that duodenal feedings inhibit these but do not inhibit the stomach contractions?

Carlson incidentally raises some of these questions and gives some experimental evidence that helps to answer them. He believes that intestinal contractions do take place during hunger, but that they play a very small part, if any, in the sensation of hunger. The proof of such a part is still wanting (4:83). In regard to question (1) above, Carlson describes some experiments made upon dogs which show that, in this case at least, stomach contractions are reflexly inhibited by stimulation of the intestinal mucosa. Intestinal and gastric fistulas were made upon twenty-four young female dogs and records of the effects of intestinal stimulation upon stomach contractions were made. It was found that gastric juice, chyme, acids, alkalies, water, milk, oil, introduced in 10 c. c. amounts into the small intestine inhibited gastric hunger contractions and gastric tonus for varying periods. "The longest inhibition obtained in any one experiment was produced by 10 c. c. of milk in the gut. In this case the inhibition lasted thirty minutes." (4:198)

The writer of this paper has no objective experimental evidence that might help in the answering of the questions given above but believes that such evidence is necessary in order to answer them and makes the following suggestions for further investigations. (1) That by the devising of some apparatus—possibly an adaptation of the apparatus used in her treatment—the presence of and the interval of periodicity of duodenal contractions be obtained;<sup>1</sup> (2) that stomach contractions be recorded during a period of duodenal feedings; (3) if it proves that periodic duodenal contractions are existent that the effect upon these be determined (a) during stomach feedings, (b) during duodenal feedings, (c) during a period of starvation.

It may well be that the duodenal feedings given the writer were frequent enough to cause a continuous inhibition of the stomach contractions and that for this reason during the greater period of the treatment she did not feel hunger. If this does explain her lack of hunger it would be interesting to determine experimentally how long the interval between

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<sup>1</sup> Carlson (4:82) mentions that in the duodenal fistula case of Busch contractions of the small intestines were noted in hunger.

duodenal feedings could be made and still have hunger absent. Whether or not the intestines play a rôle in the hunger sensation the writer cannot determine from any objective experimental evidence which she has, but that they do play such a rôle is suggested by certain introspections which she made during her period of treatment. As will be shown later in the paper, her introspective study of the sense of "fulness" convinced her that this was located in the small intestine and it was to this same location that she at times attributed "hunger." During the first week of treatment the duodenal feedings were begun at six o'clock in the morning. This generally necessitated waking her and as the feedings were begun immediately, time for introspection was lacking. On the last eight mornings of the treatment feedings were not begun until eight o'clock and on six, possibly seven,<sup>2</sup> of these mornings "hunger" was felt before the duodenal feeding was given.

Her introspections of this first morning, March 29, when she reported this early morning hunger contain the following statements: "Getting hungry: localized where the 'full' feeling is felt." On one other occasion, March 25, she wrote: "11:30. Feel a little 'hungry:' localized in the duodenum(?)." On March 28, 4 P. M. she wrote: "Before the feeding, felt fulness in the colon, also some sensation (pressure?, slight hunger?) localized in the lower stomach." On March 28, 4 P. M. she wrote: "Slight hunger: localization 'pit' of stomach." Later when she had more definitely localized the sensations in the intestine she felt that "lower stomach" and "pit" had really signified intestine. These introspections of the writer indicate that hunger may be felt at least partly in the small intestine. Perhaps it should be mentioned that at the time when the treatment was being given the previous experimental findings in regard to hunger, etc. were but vaguely known to the writer. The problem which she set herself at the time was to observe *when* she felt hunger, not *where* she felt it. For this reason she is inclined to regard her spontaneous though carefully made observations as of considerable value. During the period of the last three days hunger was reported as having been present at three times other than early in the morning but the exact time was not recorded. Her introspective notes show no localization of hunger except as quoted above. As stated before, localization was not her problem. Altogether then

<sup>2</sup> The writer did not positively identify the sensation of this morning as that of hunger.

during her two weeks of treatment hunger was felt but twelve times. It may well be that hunger was present at a few other times for strict attention to the problem was not always possible. Perhaps this is true of all hunger reports. At any rate the writer feels that her observations are on the whole truly representative.

Cannon, Washburn, Carlson, all make a good deal of the *star* relations between hunger and stomach contractions. They find that hunger is always reported *after* the beginning of stomach contractions. We have stated above that hunger was felt in the early morning before the duodenal feedings were begun and if it is true that duodenal feedings inhibit stomach contractions then this early morning hunger may have been due to the fact that the stomach contractions had not yet been inhibited because no feeding had been given during the night. But the fact that hunger was felt at other times during the day despite the fact that the time of duodenal feedings was kept constant is not so easily explained by this theory. To the writer's mind it raises the question whether or not after all the factor of "general weakness" is not a part of the hunger complex.

Hirst (7) quotes with approval Tiedemann's "admirable analysis of the sensation of hunger" in which hunger is said to be a sensation of emptiness of the stomach and the general sensation of malaise and weakness of the body as a whole. That hunger is more than local emptiness Hirst argues from the fact that in cases of prolonged starvation food relieves the emptiness but not the general feeling of malaise.

"a much larger quantity of food being required to satisfy hunger of the starved tissues than that of the empty stomach. For the former digestive and nutritive food is required, for the latter bulk is of primary importance. This is well seen in patients with a fistula of the upper part of the small intestine . . . I have myself observed one. The patient was constantly hungry, although he ate enormous quantities of food. His stomach always felt full, but the general sensation of hunger remained as most of the food escaped from the fistula with the result that the tissues continued to be starved." (7:41-2)

This general sensation of hunger Hirst thinks to be dependent upon the requirements of the tissues.

"It is increased when there are excessive owing to abnormally active metabolism, as in diabetes and some cases of Graves' disease." (7:41)

The sensation of emptiness experienced during a period of starvation Hirst believes to be partly localized in the intestines. The cause of this sensation of emptiness is best explained by motor activity.



"The sensation of emptiness in the intestines, like the corresponding sensation in the stomach, can best be explained as a result of periodical motor activity in an organ which is abnormally excitable as a result of the absence of normal stimuli for an unusually long period." (7:41.)

Carlson and Boring do not agree that weakness is a part of hunger. Both weakness and emptiness are accessories of hunger, according to Carlson. Emptiness he says is

"a peculiar feeling in the entire abdominal region. This feeling is continuous, not intermittent like the pangs of hunger. . . . It is probable that the increased tonus of the abdominal muscles in consequence of the empty state of the stomach and the intestines, contributes to the feeling in some way. . . . We question whether any part of the feeling of emptiness originates in the stomach itself. It is not so difficult to understand how a hypertonic and rhythmically contracting empty stomach may give rise to the sensations of tension, pressure, and gnawing pain. But how can it cause the sensation of emptiness, unless this feeling is merely negative of the sensation of fullness? On the other hand, if the tonus of the abdominal muscle does not suffice to maintain the normal intra-abdominal pressure when the stomach and the intestines are relatively empty and strongly contracted in hunger, the tension on all the visceral organs would not be diminished and this in turn would alter the pressure relative to the peritoneum and mesentery. If this is the factor in the origin of the feeling of abdominal 'emptiness' in hunger, the sensation should be diminished in man by lying down, in comparison with that felt when standing." (4:93-94.)

The writer of this paper has certainly always considered the consciousness of emptiness as a part of her hunger complex. As to whether or not this "emptiness" decreases upon lying down she cannot say. But in her case at least it was experienced while lying down.

As to Carlson's positive analysis of hunger, he writes: "It is more or less an uncomfortable feeling of tension, or pressure and pain referred to the region of the stomach." (4:6) He quotes with approval the following analysis of hunger given by Boring.

"Upon a background of dull pressure, which is sometimes recognized definitely as kinaesthesia or the equivalent muscular pressure, there is set a dull ache or gnawing pain which characterizes the hunger. Both pain and pressure are referred to the region of the stomach. The pain is noted as fluctuating, as rhythmical, as unstable." (4:25-26.)

Kinaesthesia, pressure, pain, seem then to be the components of hunger for both Carlson and Boring. Perhaps what the writer of this paper called "hunger" in the cases noted was not what these two would call hunger. Certainly she included in it what she calls emptiness. Also at times there was in it or perhaps one should say *with* it a consciousness of weakness. In this connection the following note from her introspections

is interesting. "April, 1. Up a little while today. Find myself very weak. Weighed, and found had lost seven pounds up to date since beginning treatment. Felt some 'part' of what have called hunger in the past: but hunger of the kind when I have felt sick. Rather bad, weak feeling; while the hunger in the morning is more localized. Is a gnawing sensation. This is a good feeling: feel fine when have it." This introspection indicates to the writer that the consciousness of weakness can be separated in introspection from the other sensations. Perhaps "emptiness" too could be so separated. But that the sensations located in the stomach should be separated out from the complex and be called "hunger" while those in the intestines should be called "emptiness" seems hardly justifiable. Probably all form a part of hunger. To the writer's mind hunger is a perception. Moreover she finds that in her everyday life what functions as "hunger" contains the sense of "emptiness" more often than it does pain. May it not be that this is a question of individual differences? The writer is reminded of Boring's analysis of appetite in which he points out that the stomach mucosa sensations which Carlson gives as a necessary part of appetite seem to be an individual affair. He writes: "In interpreting this pattern as an essential component of appetite, Carlson and Braafladt presumably put upon it their own individual meanings, which are, apparently, not completely in accord with those of other observers." (3:453) May not the same thing be true of hunger? Boring would probably dissent to this, insisting that the difference in the two cases is that in hunger we are dealing with a fusion of sensations while in appetite we have an attitude or meaning. If this is true then in the former case all normal persons would have the same components fused, while in the latter the sensory processes correlated with the attitude might vary in different individuals. The writer is raising the question whether it may not be true that hunger too is an "attitude" rather than a fusion? She certainly believes that the meaning of hunger is a constant part of hunger and she raises the question whether or not the sensory components (if one can make this distinction between meaning and sensory components) may not vary in individuals? Had she a record of her stomach contractions during her treatment she might conclude that only during stomach contractions did she feel hunger. But may it not also be true that without this sense of emptiness she would not experience hunger? Certainly she always did ~~sense emptiness~~ when she was hungry. And it is true that



she experiences what she calls hunger when pain is absent. Carlson and Boring report hunger without emptiness but always have pain in hunger. May it not be that in all of these cases hunger is present but with different sensory components? Or perhaps certain sensory components (perhaps kinaesthetic) are always present in hunger while other components vary in different individuals?

As to the consciousness of weakness the writer is inclined to agree with Carlson that this is not necessary in hunger. In her own experience, as her introspections show, hunger, at times, seemed to be accompanied by a consciousness of buoyancy and strength while at other times by that of weakness. This is also true of the hunger felt in the writer's every day life. On the other hand her introspections during treatment show that hunger was felt after exertion more than at other times.

Before leaving the discussion of hunger it is interesting to add part of a conversation with Dr. Einhorn, who feels that the need of food plays a part in the hunger process and points out the fact that in over 500 cases in which he has given the duodenal feeding treatment in very few instances is hunger reported to him. This is not true of rectal feedings where assimilation of food is much less than in the case of duodenal feeding. It may be, of course, that duodenal but not rectal feedings inhibit stomach contractions and hence the lack of hunger in the first case and the presence of it in the second case is accounted for. This is one of the points which future investigation will probably answer.

*Appetite.*—As to appetite during the treatment never did this force itself upon consciousness but came only when sight, smell, or the thought of food provoked some desire for it. This never happened however after a duodenal feeding, such a feeding seeming to "satisfy" the appetite. When appetite was felt there was decided "mouth-consciousness," i.e. kinaesthetic images of tongue movements, pressure images of "pucker," what seemed to be sensation from increased salivation. At times when the food was seen there was a motor tendency toward it. There seemed to be no gastric component present. Sometimes immediately before a duodenal feeding was given the writer watched food served and "desire" for food was often felt. In no case, however, did this arouse hunger. Introspections for 7-7.30, March 29 read: "Getting hungry: watched trays served, great desire for food tastes. 'Coffee-ah!' 'Brown rolls!' (these in vocomotor imagery)." With these images went the accompany kinaesthetic and pres-

sure images and sensations in the mouth. From her experience the writer believes that hunger and appetite are separated conscious states or attitudes. There may be mutual influence in a number of ways.

*Sensation of Fulness.*—One of the most outstanding features of the treatment was what the writer called the sensation of fulness. When the feedings were first given a rather widespread reaction occurred. Warmth (a sort of flushing) was felt over the whole body, most marked in the face and limbs. A thrilly, tingling feeling in arms and legs was rather dominant. This went up and down the limbs much as a shiver runs up one's back, but in this case it was a warm running thrill not a cold sensation, as in the case of shiver. A feeling of weakness was present and toward the end of the feeding a sensation of fulness. It is hard to describe this sensation for although it has some of the sensory components of what we sometimes term "bloating" or consciousness of gas in the intestines it had more localized muscular sensation than this. It seemed to be localized in one place, high up in the abdomen. Fulness seems the only word to express the sensation. It was somewhat like the sensation occasioned when upon washing out the stomach one feels that as much water as one "can hold" has been poured in and that it must be taken out. In both cases, i.e. in both the stomach and the intestine, fulness may pass over into nausea. In the case of the duodenal feedings there was a consciousness of pushing against the stomach, at least it seemed to be against the stomach for it was against that part where nausea was being felt. Nausea has always seemed to the writer to be localized in the stomach. At least it seemed higher up than the sensation of fulness, when they were present at the same time. Toward the end of the period of treatment the sensations of warmth, weakness, thrilling were not present in any marked degree. The sensation of fulness was present in all feedings and never was the writer able to take the feedings while sitting up. This seemed to increase the sensation of "fulness" and gave an added dragging-down sensation. It also occasioned the nausea more easily.

From experiments Hurst concludes (7:35) that the "sensation of fulness" is due to the stretching of the muscular coat and is probably shared by all hollow viscera. Carlson (4:112) points out that there must be a certain amount of tonus reaction of the stomach before tension or pressure on the walls of the stomach will produce the sensation of

fulness. The fact that the writer of this paper did not feel this sensation of fulness until the end of each feeding and that the sensation was felt more easily and markedly when the food was forced in more rapidly lends support to Hurst's contention, although one wonders if the food stays in one place long enough to give rise to muscular distention. At any rate the sensation of fulness *was* present under the conditions mentioned and apparently the small intestine is sensitive to distention. It may be mentioned that although qualitatively the sensation of fulness in the intestine resembled the sensation of "fulness" felt in the stomach after a very large meal, it was not felt in the same place.

One factor that made at least relative localization of sensations fairly easy and certain, was that the writer was experiencing fairly constant pain in the pyloric region of her stomach—due, probably, as tests revealed, to inflammation in this region. Both the sensation of fulness and hunger (at times) were localized lower than this pain. It should be stated that the writer's stomach is considerably dilated and that for this reason the localization of her sensations may be wrong. Even so the relative positions would be correct.

*Thermal Sensitivity.*—A few rough observations were made upon thermal sensitivity. Of course the tube used in the treatment was very small and single-walled thus allowing heat or cold to be quickly conducted to the surface. But as localization of alimentary sensations has been found to be pretty accurate this fact need not invalidate her observations. The writer believes that during her two weeks of treatment she became pretty competent and accurate in her ability to localize a sensation as one coming from the different parts of the alimentary canal. The strict localization of her pain and of her sense of fulness and hunger gave her what might be termed "land-marks." And then again introspection is not a new business to her.

The problem of the thermal sensitivity of the stomach seems to stand somewhat as follows. Hurst found that water from 40-50 degrees C. and also ice-water introduced into the stomach by means of an india rubber tube placed inside an ordinary stomach tube gave no thermal sensations (7:5). Boring, also using a double-walled tube, found that water at from 50-80 degrees C. produced a sensation of warmth and water at from 0-30 degrees C. a sensation of cold, both being referred to the stomach. (1:40) Carlson too concludes from experiments that "The stomach mucosa is endowed with heat and cold nerve-endings." (4:111)

From her own experience the writer believes the oesophagus, the stomach and the duodenum to be sensitive to thermal stimulation, the latter two being sensitive only when the stimulus is rather extreme, and the duodenum possibly not sensitive to warmth. In most cases the sensation of warmth occasioned by her feedings was confined to the upper part of the oesophagus. On March 28 attention was turned for the first time to this problem and introspections of that day read: "During several of today's feedings gave strict attention to temperature sensations: could sense none although when I forced the food down more rapidly it felt a little different." On March 29 introspections read: "No temperature sensations on forcing it faster." At other times when the food was given hotter than usual a vague, undefined sensation was felt in the stomach and duodenum. This was not recognized as warmth.

On the day that the tube was to be withdrawn Dr. Einhorn kindly consented to allow the writer to perform a few experiments upon thermal sensitivity. These consisted in having an experimenter<sup>2</sup> very forcibly inject a syringe full of water at varying temperatures through the tube into the duodenum. The writer opening wide her throat, held the tube away from the mouth and throat surfaces, closed her eyes so as not to know what stimulus was being given, and observed and reported the sensations experienced. One syringe full of water at each of the following temperatures was used with the following results. At 40° and at 44° Fa. unmistakable cold was sensed. This was very widespread in the abdomen and according to the writer's best introspective ability was localized both in the stomach and in the duodenum. At 54° the cold did not seem so widespread and it was doubtful whether or not it was at all localized in the duodenum. At 104°, 106°, 108°, 112° Fa. warmth was felt in the stomach but it was not as widespread as the cold and the writer felt rather doubtful about its being in the duodenum. At 115° it felt considerably "hotter," more widespread, and was possibly sensed in the duodenum, although the writer was not sure of this last point.

The writer offers these few observations on thermal sensitivity not as conclusive evidence in any sense but merely as having suggestive value. The experiments, together with her observations during the whole treatment, do incline her to the view that the stomach certainly and the duodenum prob-

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<sup>2</sup> The writer's brother, Dr. Malcolm S. Campbell, kindly performed these experiments.



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ably have thermal sensitivity. Both seem more sensitive to cold: although higher degrees of warmth might have given different results. The whole problem should be put to further experimentation.

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## PLEASANTNESS AND UNPLEASANTNESS IN RELATION TO ORGANIC RESPONSE

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In a previous study<sup>1</sup> of mixed feelings we came to the conclusion that pleasantness and unpleasantness are not felt simultaneously but only in alternation. This incompatibility indicates the existence of two opposed physiological mechanisms which are involved in affective response.

We have thought that a comparative study of the organic-kinaesthetic processes of pleasant and unpleasant feeling might throw light upon the nature of affective opposition. Accordingly we have taken for an experimental problem the following: is there any characteristic difference between the organic-kinaesthetic factors of P and U feeling which may throw light on the physiological mechanism of affection?

We have evoked feelings by simple, single stimuli: odors, tastes, tactual impressions, and a few chords. The feelings aroused vary intensively between the mild aesthetic feelings and strong emotion. Following is a complete list of the stimuli used:

*Olfactory*: wintergreen; caproic acid; creosote; castor oil; white rose perfume; asafoetida; cod liver oil; nitro-benzole; peppermint odor; camphor; anise; violet perfume; heliotrope perfume. *Gustatory*: vinegar; castor oil; chocolate peppermint candy; sarsaparilla; salted almond. *Tactual*: stroke with velvet; tickle ear; slap with lamella; sandpaper chin and nose; snap on cheek with rubber band; pain from sharp nail; extension of arm for 60 sec. *Auditory*: chords and discords.

The subject was seated in a Morris chair, eyes closed, in a room specially prepared for the experiment. Every S worked two hours a week, at approximately the same time of day; a single session took from  $\frac{1}{2}$  to  $\frac{3}{4}$  of an hour. The work was done during the winter of 1919-20, and was broken by the Christmas holidays.

The following instruction was finally adopted after several slight modifications:

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<sup>1</sup> Young, P. T., An Experimental Study of Mixed Feelings, *Am. Jour. of Psychol.*, 1918-19, 237-271.

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"In this experiment be passive and receptive. Let the experimental situation have its full normal effect upon you.

"Report all muscular tendencies and organic sensations in any way related to the affective reaction.

"Report whether the experience was pleasant, unpleasant, or indifferent; and indicate the intensity of the feeling (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong')."

Our subjects were five men and two women, all connected with the psychology department at the University of Minnesota.<sup>2</sup> We shall designate these subjects by letter: A, B, C, D, E, F, G.

TABLE I  
TOTAL NUMBER OF REPORTS

Subject	A	B	C	D	E	F	G	Total
Pleasantness....	25	17	12	6	27	26	9	122
Indifference*....	19	6	12	2	14	4	13	70
Unpleasantness..	34	29	8	8	41	17	11	148
Total.....	78	52	32	16	82	47	33	340

\*"Indifference" includes all reports in which pleasantness and unpleasantness are not specifically mentioned, as well as those in which the experience is described as indifferent.

## RESULTS

The total number of reports in the experiment is shown in Table I. It will be noticed that there is a good balance between P (122) and U (148).

Of the specific organic-kinaesthetic processes reported by our subjects the most frequent are:

Muscular strain, tension, shivers, shocks, jumps, moving waves of sensation; relaxation; incidental movements such as chewing, swallowing, tongue movements, jaw movements, eye movements, special associated movements, etc.; warmths, colds, pressures referred to the chest, heart and trunk; observed changes in respiration and circulation; perception of passive movement, of the flow of saliva, of tendency to vocalize; kinaesthesia of amusement, smiling, frowning; anger, expectation, surprise, nausea, sneezing, snarling, etc.

*Correlations.*—A preliminary survey of our data shows clearly that P and U are frequently reported without any mention whatsoever of organic and kinaesthetic accompani-

<sup>2</sup> The subjects were: Dr. R. M. Elliott; Dr. (Miss) M. Fernald; Dr. (Mrs.) J. C. Foster; Dr. W. S. Foster; Dr. J. J. B. Morgan; Mr. O. P. Pearson; Mr. C. P. Stone.

ments. Of course there may be subliminal physiological changes accompanying every P and U feeling which, conceivably, might be detected by an ultra-refined expressive method. However, peripheral changes were not observed by our subjects in a large number (38%) of reports. We conclude that there is no sensory *sine qua non* of P and U.

Consequently we must limit our investigation to reports containing specific mention of organic-kinaesthetic processes. Apparently there are a few specific correlations generally recognized in daily life. Smiling, for example, appears to be correlated with P; frowning and snarling with U; nausea (3 cases) with U, etc. There is also a suggestion of correlations between P and U and changes in respiration. However, no conclusions can be drawn safely from our data, and the correlations in question are best studied by the physiological methods.

If, however, we examine the reports more closely, we find a general correlation-tendency of considerable interest. Muscular strain (tension) is correlated with U, while its opposite, relaxation, is correlated with P. In 28 reports strain is associated with U feeling, and in 31 muscular relaxation is associated with P. The distribution of these reports is shown in Table II. This result confirms previous work.<sup>1</sup>

Following are sample reports in which muscular strain (tension) is associated with U:

TABLE II  
MUSCULAR STRAIN AND RELAXATION IN RELATION TO PLEASANTNESS AND UNPLEASANTNESS

Subject	A	B	C	D	E	F	G	Totals
Strain with Unpleasantness	7	8	0	5	0	7	1	28
Strain with pleasantness	0	2	0	0	0	1	0	3
Relaxation with Pleasantness	6	0	7	3	3	12	0	31
Relaxation with Unpleasantness	0	0	0	0	0	0	0	0
Total Reports	78	32	32	16	32	47	35	340

<sup>1</sup> Hayes, S. P., A Study of the Affective Qualities *Am. Jour. of Psychology*, Vol. 17, 1906, 358-393. "For all observers alike . . . judgments of strain were easy and, on the whole, direct. Strain was, however, described in 'muscular' terms throughout, and increasing strain meant uniformly, increasing unpleasantness." Three of Hayes' subjects make relaxation the opposite of unpleasant strain and the curves are curves of pleasantness; one other subject considers relaxation a pleasurable muscular attitude or a pleasurable organic set.



"U. mildly so . . . I noticed that my hands were crossed quite tightly with a tension that increased slightly." A 29. "That was distinctly U. There was quite a foreperiod of apprehension—a straining." A 41. "It was U, meaning that there was some tension in the muscles around the nose, possibly tending to curl it up and withdraw it." A 65. "That is U but one's main interest is in balancing the strains all over the body." A 67. "That was U, rather unexpectedly so . . . Quite a degree of tension as compared with the muscle position assumed for P and relaxing stimuli." A 71. "There was the usual degree of tension that characterizes the foreperiod of expecting U as I did then. The tension there seemed to be lateral where the tension for the U odor seemed to be vertical." A 72. "That was U. I didn't notice any particular kinaesthesia except a slight tightening of the muscles of the lower part of the nose and the upper part of the lip." B 13. "Decidedly U . . . Tightening up of the eyes—a tense closing of them." B 23. "It was U. Quite a noticeable tightening of the muscles in the lower part of the face. It seemed to be the reverse of sniffing—the attempt to prevent air from coming in." B 25. "U . . . Only a very slight tendency to tighten the muscles about the nostrils." B 27. "I should say U, but very weak . . . Tightening of the muscles about the trunk." B 40. "U, strong. Tendency to shut off the breathing. Tendency to draw head away which was inhibited by the experimental situation. Tightening of muscles about the nose. Drawing together of muscles between the brows—a slight scowl." B 42. "Very faintly U. I didn't notice anything more than a slight tensing of the muscles concerned with smelling." B 46. "I should say very weakly U. I think there was some tenseness of the muscles about the nose and thorax." B 47. "That was U, rather moderately . . . First a little excitement, a little tension of the muscles." F 2. "A sudden tensing of the muscles as soon as you struck my cheek . . . It was U, though not so very U either, mostly surprise and tensing of the muscles in the right arm and also jerking away from where you struck." F 4. "Muscles had a tendency to contract. Face frowned . . . Very U." F 24. "A tendency to make muscles more tense. It was indifferent, or if anything weakly U." F 32. "Definite tensing of the muscles, pulling away to the side, turning of the head . . . Very U" F 44. "Quite a definite tensing of the muscles especially in the upper part of the body in the arms. Tendency to lift right arm and brush it away. Feeling was definitely U. Also during rubbing there was a tensing of the stomach muscles." F 49. "The feeling was U, very intense . . . A tendency to stiffen the muscles of the arm. That was a very slight feeling of stiffening of the arm—more like just tensing the arm." G 10. "At first there was tension and trifling surprise. During that time the feeling tone would be impossible to name; neither P nor U; exciting if you want to call it so; tensing if that is a feeling tone. Then the U which increased rather up to a maximum and then decreased a little. The U itself I can't say much about . . . A tension through my face." D 2. "That was U and exciting and perhaps somewhat tensing. The tension and excitement began before the first 'now' . . . The tension and excitement so far as I could tell were simply muscular feels in the cheek. The excitement was merely little contractions and quivers and that sort of thing. The U was many strains in a tendency to shake your head and get away from it—a frowning and possible tension through the body in the legs and arms as if to help you bear

it." D 5. "U. moderately strong . . . . Something of the same kind of a shiver you get with the first reaction to cold. At the same time there was a general increase in the kinaesthetic tension, particularly noticeable in my chest, in my face and in my arms. In my chest there was kinaesthetic holding of my breath for a bit after a rather sharp inspiration. In my face there was kinaesthesia frowning and tensing all my face muscles. In my arm simply a special tension . . . ." D 10. "U. The reaction was a kinaesthetic jump of my whole body. A sudden tension and then it was all over. I should say there was tension in my legs and tension in my chest and face strongest. There was frowning. Perhaps I should also add that there is a pretty strong tension in my neck as if to draw my head away." D 12. "Mildly U. General tension through chest and in face at first. The tension kept up in my face although I kept up breathing naturally . . . . I think the general tension and the U both were even greater towards the end than towards the beginning . . . . An increase of tension—general tension—after that." D 14.

Below are sample reports in which relaxation is correlated with P:

"P. I should say quite a bit more relaxation than in any other of the stimulus periods. I should call the pleasure the absence of kinaesthesia, the relaxation, the lack of opposing tension in the muscles." A 40. "Moderately P. A singular absence of kinaesthesia . . . . I was thoroughly relaxed." A 55. "That had a forperiod of U. I was apprehensive. I was alarmed. I could feel the tension relax, particularly in the upper part of the head. My consciousness was next P by way of anticipation . . . . The P I could find little basis for except absence of disturbing kinaesthesia." A 63. "Tension followed by relaxation and deeper breathing. All initiated before I had anything P except by ideational anticipation . . . . Attention that accompanies your approach was rapidly followed by relaxation of the muscles and deeper breathing—P." A 66. "I don't know whether the P caused the relaxation." A 69. "That was P . . . . The result was ease and relaxation which spread back and surrounded the violet (odor) without diminishing the degree of P which I had from the start felt from the violet." A 70. "It was moderately P. A feeling of relaxation. It tended to increase my relaxation." F 1. "That was a P odor. It tended to make me more relaxed—to stretch." F 17. "It had a calming effect; tendency to relax. Very P." F 23. "At first a little tension but the effect of the taste was calming, relaxing, soothing . . . . It was moderately P." F 33. "It had a calming, soothing effect. A tendency to relax—was quite P." F 34. "Soothing, calming effect seemed to be localized in the front of the nose. A relaxing effect on the body. A tendency simply to let go. Very P." F 36. "A definite tendency to relax . . . . It was very P." F 40. "Definite sensations from the central part of the chest. Produced a sort of diffused feeling. A slight tendency to relax. Moderately P." F 42. "Odor had a very soothing, calming effect. A tendency to relax—very P." F 45. "I believe a slight tendency to relax although it was approximately indifferent. A weak P feeling produced." F 47. "A slight tendency to relax. Weakly P." F 48. "Mildly P. Slight relaxation." C 8. "That is mildly P. It is relaxing, reminiscent." C 11. "P. Mildly P. A rather clean and clear and relaxing kind of a thing." C 15. "The feeling tone changed.

At first it was P and then tiresome . . . . At first a relaxation and various fleeting images but that all went before the end." C 16. "There was conscious relaxation, a breathing slower and deeper in an effort to give myself up to the smell. A fleeting wonder if I would have time for a second intake of breath. My judgment now is that it was mildly P, that I would have had the same P if I had relaxed and breathed more slowly and deeply without any smell." C 21. "When I realized that it was a smell there was the usual relaxation—deep, slow breathing . . . . I think the smells give me a kind of day-dream relaxation that is mildly P. I mean it is a P way to occupy your time till something more interesting comes up. You are simply passive." C 24. "That was P. There was a general relaxation, deeper breathing." C 30. "The P was a relaxing P rather than an exciting P." D 3. "I think of the experience in parts, and tend to report that the straining parts were U and the relaxing parts or moments were P." D 6. "Slightly P. I should say that the chief thing was a bit of relaxation very mildly felt through the chest and general muscular system, considerably more through my face." D 13. "P. Sort of a soothing effect. A tendency to relax." E 11. "That was P. I think there was a feeling of relaxation when I found it was the candy and not the vinegar." E 18. "That was P, fairly intense. I didn't observe any kinaesthesia. The soothing effect is a tendency to relax more." E 43.

The tendency for strain to occur with U and for relaxation to occur with P is by no means necessary and invariable. In the first place, we have several reports of strain and relaxation in which no mention is made of either P or U. In the second place, while the straining attitudes of anticipation, sensory preparation, attention, etc. are generally U in our data, there is no *a priori* reason why such strain should not occur simultaneously with P. The three reports (Table II) in which strain is associated with P are of this sort. In every case, however, the P is very weak, and there is no guarantee of strict simultaneity of strain and P.

"I should say it was very weakly P . . . . Tendency to tenseness of some of the facial muscles (related to the questioning attitude)." B 43. "I should say it was very faintly P. I didn't notice anything in the way of response other than a slight tensing of the muscles about the nose (related to olfactory attention)." B 45. "A very little tension in the muscles. It was P, I think, though not very much—a low degree." F 3.

In the third place, we have six reports in which the subject was asked to hold out the arm for 60 sec. In three cases the strain produced was U. In the other three, however, it was indifferent or "interesting." A laboratory attitude may make

a strain mildly agreeable! Hence the U is not intrinsic to the strain,<sup>4</sup> for strain is not necessarily U.

From the above considerations we conclude that the association between strain and U, and between relaxation and P, is not necessary and invariable. In this it resembles the correlation-tendencies found by the expressive methods.<sup>5</sup>

*Intensity of response in relation to P and U.* Our data make it possible to investigate the relationship between intensity (or extensity) of bodily response and P-U. The reports contain (1) organic-kinaesthetic processes related to the affective reaction, (2) the corresponding feeling with frequent mention of its intensity. What is the relationship between P and U, of different intensities, and the number or amount of organic-kinaesthetic processes reported?

To answer this question we have given every report an 'organic score' which shows the number of separate organic-kinaesthetic processes reported. As this rating was in no case greater than four we adopted a five-point scale: 0 1 2 3 4. A few reports contain processes which apparently have no relation to the affective response, such as the incidental and accessory movements of chewing and swallowing, and random eye-movements. These movements, when reported, have not been included in the count and are not shown in the organic scores. Whenever there was any doubt as to the significance of the association between organic-kinaesthetic process and affective reaction, as in the case of heart-sensa-

<sup>4</sup> We have a large number of reports showing the dependence of P-U upon mental attitude or "brain set." "If I take the laboratory attitude, it is P no matter how U it may be. It is either P or U according to my point of view." C 9. "One difficulty is from the attitude of being in a laboratory experiment. There is a mild P all the time. Most of the experience is P from that point of view." C 19. "Neither I guess but I haven't the slightest objection to your calling it mildly either . . . I can't tell you anything about the two judgments except that evidently I have different points of view." C 21. "Potentially U but again rather interesting and interesting things are intrinsically P." C 22. "I imagine that if I didn't like doing laboratory experiments, I'd spit out the vinegar and the fact that I do swallow it means that I like it from one point of view." C 27. "That was U, of course, but it was so mixed with interest that I hate to call it U. I don't want to call it U because I was distinctly interested." 32. "I imagine you could give me pretty disgusting odors and I'd find them P. They are interesting. It is interesting to get down into an odor." A 34. "In so far as it became interesting it wasn't U." A 45.

<sup>5</sup> Leschke, Erich. Die Ergebnisse und die Fehlerquellen der bisherigen Untersuchungen über die körperlichen Begleiterscheinungen seelischer Vorgänge. *Archiv. f. d. ges. Psychol.* 1914 31, 30, finds an agreement of about 90% among previous investigators.



tions, chest-pressures, and the like, these processes *have been* included in the count. The organic score of a report, therefore, represents the number of probably significant organic-kinaesthetic processes reported. We have taken it as an index of the intensity or extensity of bodily response.

That there may be no misunderstanding as to the meaning of the organic score we give below a few examples of reports of every grade on our scale.

Reports with organic score of 0:

"Rather P. I couldn't detect any kinaesthesia there at all." E 20.

"Quite surprisingly U . . . I had expected something P because the same stimulus was P. I was surprised." A 20.

"That was P, quite P . . . The only kinaesthesia was that of the actual movements of chewing and swallowing." B 32.

Reports with organic score of 1:

"Very P and intense. A tendency to breathe deeply . . ." G 25.

"Rather U. A kinaesthetic tendency to pucker up my nose." E 22.

"U. . . a slight tendency to scowl—a tightening of the muscles about the eyes." B 12.

Reports with organic score of 2:

"Quite P. A calming, soothing effect, a tendency to relax. I wanted to inhale as much as I could. Organic sensations from the heart and chest, moderately diffuse . . ." F 34.

"Not so very U . . . I was aware of the usual pinching, withdrawing response. The whole situation was sufficiently marked to produce reverberations that accompany most of the U stimuli . . ." A 45.

"That was very U. A tension of the muscles, especially those of the face and mouth. The muscles in the rest of the body were also made more tense and I seemed to get that feeling of tension toward the stomach." E 8.

Reports with organic score of 3:

"Decidedly U. A definite jerking back of the head. Tightening up of the eye muscles—a closing of them. I was aware of circulatory changes localized in the spot struck and I should guess that there was a rush of blood to the face for that particular spot felt hot." B 23.

"Moderately U. The first tendency was to refrain from breathing. The second was to move my head away. There was also a tendency to contract the muscles about the nose and upper lip." B 49.

"It was rather relaxing at the most P part. At other times I think it was mainly exciting but not relaxing . . . It is true that there were curious little quivers and trembles and reverberations through my fist and it is true that there was a tendency to smile but it does not seem correct to say that they *were* the P but rather that they were the expression of the P as well as of the relaxation and the excitement." D 8.

Reports with organic score of 4:

"That was distinctly U. There was a foreperiod of straining apprehension and a kinaesthetic tendency to look up . . . The rap was very sharp and I felt a twitch throughout the length of my body . . . The reflex was followed by one of those warm waves that goes over you—an unpleasant keying up of the body . . . There was distinctly a vasomotor change . . ." A 41.

"That was very U. A tension of the muscles especially those of the face and mouth. The muscles in the rest of the body were also made more tense. I also seemed to get that feeling of tension down towards the stomach. I seemed to get a taste of the liquid in the oesophagus. It seemed to be something going down—U." F 8.

TABLE III  
AVERAGE ORGANIC SCORES

Pleasantness				
	Weak	Average	Strong	No Report Intensity
Number of Reports.....	34	29	31	28
Their Total Organic Score.....	25	23	23	16
Average Organic Score.....	0.7	0.8	0.7	0.6
Unpleasantness				
	Weak	Average	Strong	No Report Intensity
Number of Reports .....	47	29	27	45
Their Total Organic Score.....	45	45	61	43
Average Organic Score.....	1.0	1.5	2.2	0.9
Indifference				
Number of Reports.....	70			
Their Total Organic Score.....	37			
Average Organic Score.....	0.5			

Table III shows the average distribution of the organic scores. From it several conclusions may be drawn.

First, the average organic scores for U are in every case greater than those for P. If we take the total P reports as a class, we find an average organic score of  $0.7 \pm 0.6$ ; the same for U gives  $1.3 \pm 0.7$ ; and for I,  $0.5 \pm 0.7$ . Although the

M.V.s are relatively large, indicating that the organic response has a considerable range of intensive variation, they are practically constant for all classes of report. The total number of reports upon which these averages are based is: P, 122; U, 148; I, 70 (Table I).

Secondly, the organic scores for U increase with the intensity of feeling. The more intense the feeling, the more widespread is the bodily reaction. For P, on the other hand, no such relationship appears. The average organic scores for P are practically constant for all intensities and classes of P. The 31 reports of intense P have exactly the same average score as the 34 reports of weak P.

Thirdly, the average organic score for the reports of indifference is less than any score for P or U.

When no intensity is reported the organic scores lie midway between weak feeling and indifference, and we hazard the guess that the feeling-intensity of this class of reports is, on the average, very weak. As the intensity of P and U decreases, the organic scores approach that of I.

We should note that relaxation is included in the organic scores of P as a significant process. Some of our subjects describe relaxation as the "letting up of strain," "the absence of kinaesthesia," *i.e.*, they tend to regard relaxation negatively as the release of strain.<sup>6</sup> Suppose that we assume that relaxation is negative, merely the release of strain, and on the basis of this assumption eliminate relaxation from the organic scores. If we do this, the organic scores for P become equal to that of indifference. Hence, with our data, P is differentiated from indifference primarily by relaxation, which itself is negative and represents a return to indifference (from one point of view) rather than a departure from it.

If, for the sake of comparison, we eliminate strain from the U reports, the average organic score is reduced from 1.3 to 1.1; and the latter is well above indifference (0.5).

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<sup>6</sup> Shepard, John F., Organic Changes and Feeling, *Am. Jour. of Psychol.*, 1906, 17, 522 ff. "Strain is described as composed of sensations from the muscles, the backflow from the acting muscles, particularly those of accommodation of a sense organ. . . . Relaxation seems to be a release from either strain or excitement." Cf. Titchener, E. B., Zur Kritik der Wundt'schen Gefühlslehre, *Zeits. f. Psychol.*, XIX, 1899, 321 f. Our result confirms the later polemical work against the Wundtian theory as regards the nature of strain and relaxation.

TABLE IV

## AVERAGE ORGANIC SCORES WITH CUTANEOUS STIMULI ELIMINATED

Pleasantness				
	Weak	Average	Strong	No Report Intensity
Number of Reports.....	34	27	29	28
Their Total Organic Score..	25	20	23	16
Average Organic Score.....	0.7	0.7	0.8	0.6
Unpleasantness				
	Weak	Average	Strong	No Report Intensity
Number of Reports.....	38	13	11	23
Their Total Organic Score..	35	17	27	15
Average Organic Score.....	0.9	1.3	2.4	0.65
Indifference				
Number of Reports	55			
Their Total Organic Score	27			
Average Organic Score	1.3			

It may be objected that the foregoing result is due to the selection and balance of stimuli. The cutaneous stimuli (slaps, punches, etc.) were for the most part U while we had no stimuli capable of evoking correspondingly intense P. Had we used other stimuli and more intense P situations the differences of the curves might disappear.

To test this criticism we have eliminated all cutaneous reports and worked out average organic scores for the remaining noise tones and chords (Table IV). We believe that simple noise tones and chords are comparable from the affective standpoint. The elimination of cutaneous stimuli reverses the P-U balance of 1.08 to .65. See Table IV. However, the conclusions still stand: the differences are even



clearer than before. The U curve is steeper. Consequently the presence of a relatively large number of U cutaneous stimuli does not account for the differences in the organic scores.

As regards intensity, we have no means of comparing P with U. We do not know whether the "intense P" of a perfume is equal to the "intense U" of caproic acid, and whether the "moderate P" of peppermint-candy is comparable with the "moderate U" of vinegar. It would be worth while to study these organic curves at higher levels of feeling-intensity. We may conjecture that the P curve would rise slightly and gradually, and that the U curve would rise suddenly and abruptly to a point much higher than the most intense P would reach.

We should note, however, that the relative numbers of "weak" "average" and "strong" Ps and Us are about the same (Table III). The intensive balance of our reports is very good.

*Fundamental characteristics of the unpleasant response.* An analysis of our data brings out several fundamental characteristics of the U response.

First, there is the well-known tendency to withdraw oneself from the stimulus-object, either reflexly or deliberately.

"Much the strongest avoidance impulse so far . . . It was all I could do to keep from brushing it away." A 38. "U. Quite a definite desire to withdraw and prevent one's face from being damaged." A 64. "A decided tendency to draw my head back which was not inhibited in this case." B 15. "I got away from it. The kinaesthesia was a definitely overt action." B 28. "Slightly U. I reduced the intensity of the sensation by drawing my head away." B 37. "U, strong. A tendency to shut off the breathing—to reduce it. Tendency to draw head away . . ." B 42. "Moderately U. The first tendency was to refrain from breathing; the second was to move my head away." B 49. "The head was jerked aside with the blow. It was moved to the left." G 32. "A very strong tendency to turn the head away from the odor." F 14. "First just a little start or shock. A tendency to draw the head up and back away from the spot rubbed." F 30. "Immediately there was a tendency to jerk my head away and to push away with my right hand." F 35. "A great deal of kinaesthesia particularly in my eye-lids as if protecting my eyes from a blow and a general tendency to shrink away." C 2. "A tendency to stop up my ears and to draw away from the stimulus." C 9. "The U was many strains in a tendency to shake your head and get away from it—and frowning and possibly a tension through the body in legs and arms as if to help you bear it." D 5.

Secondly, closely related to the above is the tendency to put the object away from oneself or to prevent its action. Bio-

logically this type of response has the same purpose as the above.

"Provoked kinaesthesia of pushing the stimulus away—in the left arm in particular." A 19. "It was all I could do to keep from brushing it away." A 38. "The organics of an odor like that lead one to reflexes of repulsion." A 42. "U. Quite a noticeable tightening of the muscles in the lower part of the face. It seemed to be the reverse of sniffing—an attempt to prevent air from coming in. If I'd been wholly natural, I'd have closed the nostrils with my hand." B 25. "U. rather strong intensity. I got a movement of my intestines, a sort of nausea. It was an expulsive movement preliminary to vomiting." E 51. "I had a tendency to move my mouth to blow it away. It was disgusting. I thought I got a little nausea." E 56. "A tendency to lift up the right arm and brush it away. Tensing of neck and jaw muscles." F 49. "A tendency to stop up my ears and a tendency to draw away from the stimulus." C 9. "I tended to spit it out. Then the laboratory attitude came back . . . . If you get too naive and too relaxed you have a physical reaction and nothing else. I think that time I would simply spit it out." C 14. "I am perfectly sure that under other circumstances I should have actually reached up and knocked away the stimulus from my chin." D 10.

Thirdly, there is a tendency to inhibit or resist (frequently due to the experimental situation) some normal response to the stimulation.

"A tendency to keep it in my mouth rather than to swallow it." B 18. "U. There was a much weaker tendency to resist smelling than in the other case. Only a slight tendency to tighten the muscles about the nostrils to the extent that it interfered with breathing." B 27. "I tended to inhibit the movement of swallowing. I think there was some tenseness about the nose and throat which may have been connected with my inhibiting of the swallowing." B 47. "The first tendency was to refrain from breathing. The second was to move my head muscles and to hold the head in position against any increase in pressure." B 15. "There was some resistance to swallowing although it was swallowed." G 20. "A tendency to resist the stimulus." E 27. "Slight stiffening of the neck to hold the head rigid as the friction was increased." G 18. "A slight tendency to shut out the noise . . . to put my fingers in my ears." G 30. "There was a movement in my cheek muscles and my facial muscles producing a frown and a reaction against the stimulus in the opposite direction." E 48. "There was a rivalry between the tendency to move the head away and to hold it there." E 61. "Kinaesthesia was localized in my chin—a resistance to the rubbing. A tendency to pull away from the placed rubbed." F 6. "I had to force myself to hold the chin still. A tendency to withdraw my chin from the rubbing." F 9.

Muscular strain or tension is frequently present in the form of anticipation of a U stimulus. From one point of view such strains may be regarded as the resistance to an expected undesirable situation.

Fourthly, the unpleasant response is accompanied by reflex twitchings, convulsive contractions, and bodily reverberations of various sorts, especially when the stimulus-object is presented suddenly and there is an element of surprise.

"At first a bit of suspense; then I jumped (marked reaction). Consciousness was empty there and the first thing I remember was the sensation from the jump . . ." C 10. "No feeling tone at first during the violent reaction, the jump. Then it began to tingle . . . There was a desire to slap back." C 25. "A little shiver of what one might call revulsion went through me. The shiver itself seemed to start in my chest and go even down into my legs. It was something the same kind of a shiver you get with the first reaction to cold." D 10. "The reaction was a kinaesthetic jump of the whole body. A sudden tension and then it was all over (S's face seen to twitch)." D 12. "At first a little shock. I didn't expect the sounds. It seemed to be spread down toward the fingers, the arms." F 3. "A slight jar or shock at the beginning of the playing. The shock was felt in the heart and chest." F 16. "A sudden start. A sudden tension of the muscles as soon as you struck me." F 4. "Quite a definite tensing of the diaphragm and a jump in the hand which tapered off rapidly." A 7. "Distinctly U. It starts reflex twitchings in every part of the body, particularly in your hands but also in your head and around your ear. Quite a mass of tendencies to react." A 22. "At first a start that is so reflex that it is done for you. Then after a slight pause the wave of sensation. It is a wave coming from my feet and rises higher and higher and then recedes." A 26. "Quite decidedly U. I very distinctly felt my heart give a thump. There was a wave-like twinge that goes up your back. This time I was conscious of it going down my right leg." A 36. "I felt a twitch throughout the length of my body. The reflex followed by one of those warm waves that goes over you—a keying up of the body." A 41. "The whole situation was sufficiently marked to produce reverberative reflexes that accompany most of your U stimuli." A 45. "I felt movement down my neck and chest as well as on my cheek (marked reaction)." E 33. "I had a movement in the pit of my stomach right beneath the floating ribs." E 39. "U, very intense. I got movement in my neck and facial muscles, twitchings (seen)." E 52. "There was a feeling as if perspiration came out. A muscular reaction—movement of the head." E 79.

Summing up the characteristics of the unpleasant response we may say that, first, there is a tendency to react away from the stimulus-object; secondly, a tendency to put the stimulus-object away from oneself or to prevent its action; thirdly, a tendency to inhibit or resist the normal response (tension); fourthly, bodily twitches, shocks, waves of sensation, and other reverberative reflexes.

*Analysis of the P response.*—While U is associated with a large variety of reflex movements, P is passive and negative. Our data do not contain a single case of active reflex response to a P stimulus. With U there are withdrawing

movements, frowning, straining, reflexes of expulsion, etc. while P is characterized by mere acceptance of the situation and the passive yielding to it.<sup>7</sup> Also the bodily reverberations which are associated with U (twitches, shocks, waves of sensation, etc.) are entirely absent from the P reports. U is organically positive and active while P is negative and passive. This general finding brings into relief the significance of relaxation. Muscular relaxation is the typical process associated with P and, so far as our data go, both qualitatively and quantitatively, differentiates P from indifference. Relaxation itself is negative and passive, a letting-up of strain.

The traditional relation between P and seeking movements finds little support in our reports, while that between U and withdrawal is abundantly confirmed. The entire evidence for seeking movements, as 'expressions' of P, is found in 11 reports, in which the subject "sniffed," "took a deeper breath in order to get more," "held the breath," etc. In every case these seeking movements are deliberate and never reflex like most of the withdrawing movements of U. It is a question to what extent and in what sense voluntary, deliberate behavior based upon the knowledge that P may be produced or prolonged, or U avoided, can be considered an 'expression' of feeling.<sup>8</sup>

#### CONCLUSIONS

Let us now ask the original question: is there any characteristic difference between the organic factors of P and U feeling which may throw a light on the physiological mechanism of affection?

I. A study of the processes reported by the subjects shows that there is no organic-kinaesthetic *sine qua non* of affection. P and U are reported in more than a third of the cases without any mention of organic-kinaesthetic processes; and when

<sup>7</sup> "I was willing to have the thing continued." A 4. "A relaxing effect on the whole body. A tendency simply to let go." F 36. "It is the kind of a thing you want continued." C 16.

<sup>8</sup> If we look at the question theoretically, it is clear that in many cases seeking movements are expressions of U: strong hunger, for example. The situation is complicated by the fact that the total seeking activity may become P, as when prey is sighted or food smelled. With the seeking movements of sex there is room for argument. Unsatisfied and unrestrained desire is probably U when associated with seeking. However, there may be vaso-dilatation, general relaxation, anticipatory imagery, etc., so that the total activity of seeking is P. Since positive seeking movements may be associated with either P or U, the traditional coupling of P with seeking is unsatisfactory.

such processes are reported, they bear no fixed and invariable relationship to the affections. However, a number of tendencies toward correlation can be made out, and of these the most probable is that between muscular strain and U and muscular relaxation and P.

II. A statistical study of the organic-kinaesthetic processes in relation to affection and an analysis of the reports themselves bring out the fact that: U is associated with a positive bodily response which becomes more intense and widespread as the feeling becomes stronger, while with P the bodily response is relatively slight. When one reacts away from the stimulus-object, or puts it away from oneself, or resists it (strain), or when bodily 'reverberations' are present, U is apt to be felt. P, on the other hand, so far as our data go, is organically-kinaesthetically negative. P is felt when one relaxes, or simply 'does nothing;' there are no reflex responses to the stimulus-object and no bodily 'reverberations.'

It should be remembered that any result is a function of conditions. Our subjects were seated quietly in a Morris chair, instructed to be "passive and receptive" and "to let the experimental situation have its full normal effect." In one of the earlier instructions they were asked to "make no resistance to the stimuli; to let them have their full effect." The *Aufgabe*, therefore, was to accept the stimuli calmly and quietly, or else as one does in ordinary life. With this set simple odors, tastes, tactual impressions, and a few chords and discords were presented. Those responses which are described as U involve a positive reaction which is absent with P.



# AN EXPERIMENTAL STUDY OF KINAESTHETIC IMAGERY<sup>1</sup>

By ALICE HELEN SULLIVAN

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## INTRODUCTION

In the first part of this paper we seek to determine the differences between sensory and imaginal kinaesthesia. It is often said that the kinaesthetic sensation and the kinaesthetic image are difficult or impossible to distinguish. We have attacked the problem by way of simple kinaesthetic complexes, experienced in their sensory reality and reproduced in imaginal terms. The results of this comparative study, attributive (sensational) and perceptive, are set forth on p. 67.

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In the second part of the paper we have compared resident kinaesthesia with the projected kinaesthesia of the empathic experience. The results appear on page 78.

# I. KINAESTHETIC SENSATION AND KINAESTHETIC IMAGE COMPARED

We wished to obtain, for the purpose of comparison, a psychological picture of sensory and imaginal kinaesthesia, first in attributive and then in perceptive terms. We made out 50 short commands to be read to the *O*'s. We paired these commands for sensation and image thus: (1) "Think of: nodding your head once." (2) "Nod your head once." (3) "Raise your index finger once." (4) "Think of: raising your index finger once." In order that the *O* should not know when to expect a sensory or an imaginal stimulus, we arranged the experiments by this schema: IS IS IS SI SI II SS IS IS IS.<sup>2</sup>

*O* sat in a dark room, facing a black wall. *E* sat at a table a few feet behind *O*, and near a window through which a streak of light, just sufficient for his needs, was allowed to shine.<sup>3</sup> After *O* had read the instructions, *E* asked him to close his eyes. After a few minutes of rest, to free the eyes of *O* from possible images, *E* said "Ready," waited an instant, and read the stimulus-sentence. As he spoke the last word of the sentence, which was always "once," *E* started a stopwatch. When *O* said "Yes," indicating that the command had been obeyed, the watch was stopped.<sup>4</sup> Then the complete report of *O* was taken down. An interval of 15 sec. then elapsed before the giving of the next stimulus-sentence.

We began the experiments in May, 1919, and carried them on during the following months of June (with the exception of a two weeks' vacation), July, August, and October. There were seven *O*'s: Dr. L. B. Hoisington, instructor in psychology; Drs. H. G. Bishop and M. J. Zigler, assistants in psychology; Mr. P. J. Cavanaugh, Misses C. Comstock, R. Stutsman, and A. H. Sullivan, all graduate students in psychology.<sup>5</sup>

## Experiment I.

We performed preliminary experiments, in order to try out our 50 paired commands, and to give our *O*'s practice in describing their processes. We noticed very soon that, in the

<sup>2</sup> I = Image; S = Sensation.

<sup>3</sup> A small shaded light was used for evening experimentation.

<sup>4</sup> We have not included the times in the following results, because we found no typical times for sensation and image, the time for the carrying out of the sensory and imaginal commands being approximately the same.

<sup>5</sup> St observed only during the summer term and did not complete the perceptive experiments. When the writer (Su) acted as observer, the experimenter (Ca, and later Miss M. F. Martin) made out new sets of stimulus-sentences.

pairs designated in the schema as SI, all *O*'s showed a tendency to be influenced in the imaginal part of the experiment by the report made upon the preceding sensory complex. The finding of attributive characters in the sensory complex suggested that the same attributes would appear in the image. An effort on *O*'s part not to be thus biased resulted often in a complete inhibition of imagery.

### *Experiment II*

In the light of these preliminary experiments, we settled upon the 15 paired commands which seemed simplest and yet were adequate for our needs. We also separated and distributed the sensory and imaginal members of the pairs, so that the schema SI SI SI etc. still held good, except that the imaginal member of a given pair preceded the sensory by a number of sentences.

The instructions read: "You are to follow the instructions which I shall give you and to report the experiences thus set up with especial reference to kinaesthesia.

"Every kinaesthetic experience may show perceptive characters (locality, pattern, etc.) and attributive characters (quality, intensity, etc.). In the present series I wish you to report the experience in terms of quality, intensity, and extent.

"You may neglect every other character of the kinaesthetic experience, and you may neglect all non-kinaesthetic experiences aroused by the presented stimuli.

"If, however, you find yourself noting any further feature of the experimental consciousness, I shall be glad if you will report it."

It was necessary to fractionate the experiences for report. In Group I we asked for quality, intensity, and extent; in Group II, for intensity, extent and duration; in Group III, for extent, duration, and vividness; in Group IV, for duration, vividness, and locality; in Group V, for vividness, locality, and temporal course; in Group VI, for locality, pattern, and course; in Group VII, for quality, pattern, and course. In this way we covered the attributive and perceptive characters a number of times, using always the same 30 stimulus-sentences. We took one practice-series with every new group, in order that our *O*'s might attain a fair degree of stability of attitude.

Stability of attitude was not attained at once. All *O*'s with the exception of Su were more familiar with visual imagery than with kinaesthetic. Visual images came constantly at first, and acted as a distraction. After training in the observation of kinaesthetic processes, much of the visual imagery dropped into the background, and the kinaesthetic processes were sufficient to carry the whole meaning of an imagined movement. Then the *O*'s showed more certainty about their kinaesthetic imagery. After training, the *O*'s were able also to describe more accurately the kinaesthetic sensory processes.

Sources of error due to wrong attitudes had also to be discovered and eliminated. Some *O*'s fell into the habit of holding or building



up the image, instead of letting it run its course naturally, as they did in the case of sensations. We had to make it clear to them that such "holding" was possible only by a shift of attitude.

Another source of error was that of using a different scale for sensation and image in reports upon the intensive attributes. We explained that the intensive characters mean distances from zero, and that the same scale could therefore be used in both cases.

### Results

We found that, whereas the actual perception of movement aroused by our stimuli was very complex, having a number of different sensations whose attributes had to be reported upon separately, the image of the movement was in some cases for all *O*'s, and in most cases for a few *O*'s, actually a single image-process of one quality, intensity, etc., which carried the whole meaning of movement. An actual nod, *e.g.*, aroused several sensations; the thought of nodding aroused, in many instances, one simple image. In other cases, the attributive reports seemed to indicate a single image-process, but the perceptive reports which came later showed that the apparently simple process was in reality an imaginal complex, with a pattern and temporal course, which the attributive *Aufgabe* did not always bring to light.

We have tabulated our results in parallel columns for sensation and image.<sup>6</sup> There was no mention of the terms image and sensation in the instructions; and yet our stimuli were cut closely enough so that all *O*'s reported, with as much assurance as they would show in reporting upon visual sensations and images, "I observed a kinaesthetic image," or "I experienced kinaesthetic sensations." If the *O*'s indicated any doubt whether a slight movement had been made in the carrying out of an imaginal command, we threw out the results. The columns for sensation, then, represent reports of the experiences aroused by our sensory commands, but at the same time they represent the experiences which were introspectively recognized as sensory; while the columns for image represent reports of the experiences aroused by our imaginal commands, but only those experiences which were recognized introspectively as imaginal.

<sup>6</sup> In the tables, the figures within the columns show the frequencies of report for the rubrics which head the horizontal columns. If, *e.g.*, the number after *O*'s initial is 3, this means that the 30 stimulus-sentences were repeated 3 times for report upon the character in question; 45 experiments for sensation and 45 for image were performed.

(1) *Quality*

TABLE I

## QUALITY

	B (3)		Ca (3)		Co (3)		H (3)		St (1)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Du. P. ....	49	40	26	29	18	33	17		19	11	17	9	53	34	199	157
Br. P. ....							35						6		41	
Drag. ....							6						2		8	
Sm. P. ....	10	3	23	3	19	2	34		16	9	31		24	8	157	25
Strain. ....	28		28		35		38		9		20		31		189	
Ache. ....	15		9		5		11				9		1		50	
Lt. P. ....	12	5	11	29	6	13	9	47			20	38	17		75	132
Total. ....	114	48	97	62	83	48	150	47	44	20	97	47	134	42	719	314

Du. P. = Dull Pressure; Br. P. = Bright Pressure; Sm. P. = Smooth Pressure; Lt. P. = Light Pressure (or Neutral Pressure.)

Ca reported also strain in the image once in the early part of the experiment. Z reported 3 cases of strain and 3 of bright pressure in the images. The following were also reported in early observations: sharp pressure, Z, for image 2, for sensation 4; granular pressure, Z, for image 1, for sensation 1; coolness, Co, for sensation 2; St, for image 1; dizziness (which later was found to be nothing but dull pressure), for sensation, Ca 5; St, 1; Su, 3; for image Su, 2.

We see that the characteristic qualities in kinaesthetic sensations are dull pressure, light pressure, smooth pressure, strain, and ache. These were reported by all *O*'s except St, who reported no light pressure. Dull pressure averages the highest frequency, although the individual reports show that Ca, Co, and H reported the greatest number under strain, and Su the greatest number under smooth pressure. H and Z reported also bright pressure and drag. Bright pressure is experienced only when the *O* is in very good physical condition. Drag is very like dull pressure, and may have been considered qualitatively the same by the other five *O*'s.

The table shows very strikingly the fact that the image is less varied in quality than the corresponding sensations. The headings for quality run thus:

Observer	Sensation	Image
B	5	3
Ca	5	3
Co	5	3
H	7	1
St	3	2
Su	5	2
Z	7	3

In every case a greater number of qualities belongs to sensation.

Table I shows also the complexity of a perception of movement as compared with the imaginal reproduction of that movement. All the *O*'s except St performed 45 sensory experiments and 45 imaginal experiments on quality. The numbers in the total at the bottom of the table show that the different *O*'s reported from 83 to 150 separate qualities in the 45 sensory experiments, while they reported from 42

to 62 separate qualities in the 45 imaginal experiments. When we remember that 45 is the lowest number possible if we have one image for every experiment, it becomes evident that in most cases one image carried the whole meaning of the movement, provided that we take one quality to mean a simple process.<sup>7</sup>

TABLE II  
INTENSITY

	B (2)		Ca (4)		Co (4)		H (2)		St (2)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Strong.....	20		16		16		7		24		15		17		115	
M. St.....	22		5		3		17		6				21		7	74
Mod.....	11	7	63	40	58	27	22		31	4	28	10	20	18	233	106
M. Wk.....	8	16					5	10	1			3	14	16	28	45
Weak.....	13	14	48	45	33	23	25	16	11	12	39	21	6	2	175	133
V. Wk.....			3	6	3	22		5	3	18		11		3	9	65
Total.....	74	37	135	91	113	72	76	31	76	34	82	45	78	46	634	426

M. St. = Moderately Strong; Mod. = Moderate; M. Wk. = Moderately Weak  
V. Wk. = Very Weak.

The following were reported in the early part of the experiments, and were not reported in later observations: kinaesthetic image, strong in intensity, Ca, 1; Co, 7; Z, 2; moderately strong, St, 1; moderate, H, 1; kinaesthetic sensation, very weak in intensity, H, 2; Su, 1; Z, 1.

All the O's but H reported dull pressure for the image; all but St and Z, light pressure; all but H and Su, smooth pressure. The smooth pressure has so low a frequency as compared with the other two qualities that we must doubt its validity as a separate quality for the image (cf. p. 73). We may be fairly certain, then, that the characteristic qualities in kinaesthetic imagery are dull pressure and light pressure; we may say without any doubt that all qualities in the image are "pressury" (a term used by O's in describing the quality of images).

<sup>7</sup> For some observers (H and generally Z) one quality was to all intents and purposes a simple process, but for others it was not. We must not be misled by quality, for even though there is but one quality present, we have no guarantee that the image is not complex. There are still possible variations of intensity and vividness, as well as changes in temporal course. We found, indeed, that quality often-times played a surprisingly minor rôle in kinaesthetic experiences. Quality is usually regarded as the individualizing attribute; but our O's were often able to describe the intensive attributes of a sensation or image when they could not describe its quality. Furthermore, the quality of the kinaesthetic images was monotonous in its sameness, and yet the image was varied and interesting because of slight shadings of intensity, or slight changes during its brief temporal course.

The table shows only 42 imaginal qualities reported by Z in 45 experiments. In reality he reported 51 qualities, but the other 9 are included in a footnote to the table, because they are not qualities reported by other O's and were reported by Z only in early experiments.

There is neither strain nor ache in the image, though the kinaesthetic image may mean a movement that would be strainful or achy in sensation.

There were indications that there is a difference in brightness in the qualities of sensation and image. All qualities in the sensations seem to have a certain "liveness" or brightness which the qualities in the images lack. H stated the difference thus: "The quality of the image is dead and static, while the quality of the sensation 'wells up,' has brightness or liveness or sparkle, and is dynamic." There is, of course, always the possibility that an obscure perceptual motive has colored the sensations.

### (2) *Intensity*

We find that the intensity of kinaesthetic sensations ranges from strong to weak. The whole intensive scale is represented in all the reports, save that three O's report a few sensations of very weak intensity, while four report none below weak.

The highest frequency of report for B and Z occurred under the rubric "moderately strong;" for Co, Ca, and St under "moderate;" and for H and Su under "weak." Thus, the middle portion of the scale was most favored.

The kinaesthetic image has less range over the intensive scale than the sensation, going only from "moderate" to "very weak." There is one exception to this statement: Z reported 7 images as "moderately strong." Z showed a tendency to estimate all the intensive attributes high. This may have been due to the fact that he "held" his images at first; for the tendency disappeared in the later experiments.

The highest frequency for B's images was in the "moderately weak" group; the highest frequency for Ca's, H's, and Su's images was in the "weak" group; the highest frequency for Co's and Z's was in the "moderate" group. For all observers except Z more than half of all the images reported belong to the lower half of the scale. If we consider the totals at the left of the table, we see that the highest frequency for sensation fell under the "moderate" group, and for image, under the "weak" group.<sup>8</sup>

We may not say, then, that kinaesthetic images are weaker in intensity than kinaesthetic sensations, for some kinaesthetic sensations are weak; but we may say that kinaesthetic images are never strong in intensity, as kinaesthetic sensations often are.<sup>9</sup>

<sup>8</sup> We cannot expect the individual reports to agree absolutely, for our O's were using subjective scales. The small divergences from the average for the group show, however, that these scales must have been very much alike in all cases. In general, it is not the absolute estimate that we care for, but the relative judgments which show the difference between sensation and image.

We do not have agreement of numbers in the grand totals in the different tables. The characters for report were fractionated, and were repeated a different number of times.

<sup>9</sup> The O's, with further training, confined their reports on intensity of images more and more to the lower part of the intensive scale (cf. p. 73).

(3) *Extent*

TABLE III

## EXTENT

	B (2)		Ca (5)		Co (4)		H (2)		St (2)		Su (5)		Z (6)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
V. Ex.....	12		2		8		13				4				39	
Ext.....	14		40	13	40		18		10		41		74	12	237	25
M. Ex.....	10	2	19	11			7		11	2	29		15	4	91	19
Mod.....		4			34	15	6		9	10		3	54	36	103	68
M. Lm.....	6	7	8	5	6	8	5	2	1		17	22	1	2	44	46
Lim.....	8	9	35	31	1	36			19	5	14	37	37	35	121	179
V. Lm.....	6	13		2		5		12	6	5		15	6	15	18	67
Total.....	56	35	104	62	89	64	49	33	42	31	128	77	185	102	653	404

V. Ex. = Very Extended; Ext. = Extended; M. Ex. = Moderately Extended; Mod. = Moderate; M. Lm. = Moderately Limited; Lim. = Limited; V. Lm. = Very Limited.

The following were also reported; kinaesthetic image, very extended, Ca, 2; Co, 1; extended, Co, 2; St, 1; kinaesthetic sensation, very limited in extent, Ca, 2. All of these reports occurred in the early part of the experiments.

The extent of kinaesthetic sensations ranges from very extended to very limited. All *O*'s but one favored the rubric "extended" the exception being St who favored "moderately extended." Twice as many sensations are found in the upper portion of the scale as in the lower.

The extent of the kinaesthetic image varies within smaller range than that of kinaesthetic sensations. To be sure, the table shows that two *O*'s reported a number of their images as "extended." It was in this group of experiments, however, that we discovered the same *O*'s "holding" their images. After we had instructed them that there must be no voluntary holding or building up of the image, the reports of "extended" dropped out almost entirely. In spite of this source of error, we see that by far the greatest number of images fall in the lower part of the extensive scale, and that no images reach the maximum of extent attained by some sensations.

Most of the images reported by B were "very limited;" most of those reported by Ca, Co, H, St, and Su were "limited;" while most of those reported by Z were "moderate." When all the sensations reported by all the *O*'s are totalled, the highest frequency falls under the rubric "extended;" when all the images reported are totalled, the highest frequency falls under the rubric "limited."

We may say, then, that kinaesthetic sensations tend to be extended, while kinaesthetic images tend to be rather limited. We may say also that whereas kinaesthetic sensations are sometimes limited in extent, kinaesthetic images are never maximally extended.<sup>10</sup>

<sup>10</sup> The *O*'s, with further training, agree more and more in their reports upon the extent of the image (cf. p. 74).

(4) *Duration*TABLE IV  
DURATION

	B (2)		Ca (5)		Co (5)		H (3)		St (2)		Su (5)		Z (5)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Long.....	15		25	35	30		32		15		31		74	14	222	49
M. Lg.....	11		39	27	71	20	12		4		47		8		192	47
Mod.....	12	10					2	2	15	6		3	24	5	53	26
M. Sh.....	14	6	58	8	6	19	2	2		3	17	13	16	12	113	63
Short.....	4	15		28	5	26	12	34	7	6	34	46	16	27	78	182
V. Sh.....			4	2		9		8	3	11		13	5	15	12	58
Total.....	56	31	126	100	112	74	60	46	44	26	129	75	143	73	670	429

M. Lg. = Moderately Long; Mod. = Moderate; M. Sh. = Moderately Short; V. Sh. = Very Short.

The following reports occurred in the earlier experiments, and dropped out entirely in later experiments: kinaesthetic image, long in duration, Co, 2; H, 1; St, 2; moderately long, H, 1; Z, 1.

The duration of kinaesthetic sensation may be long, medium, or short. The sensations, however, tend to group themselves about the upper half of the scale, more than 50 per cent of all the sensations reported being above moderate.

The highest frequency fell under the heading "long" for B, H, and Z; under "moderately long" for Co and Su.

The kinaesthetic image, according to the table, may be anywhere from "moderately long" to "very short" in duration. This statement excludes the reports of Ca and Z, who in the first 3 repetitions of this group of experiments, "held" their images. After the instruction to let the image run its course naturally, the reports of "long" dropped out. For all O's but Ca, more than half of all the images reported are in the lower part of the scale.<sup>11</sup>

As to highest frequencies, we find the greatest frequency occurring under the rubric "short" for all O's except Ca. In the totals, the highest frequency is under "long" for sensation, and under "short" for image.

(5) *Vividness*TABLE V  
VIVIDNESS

	B (2)		Ca (3)		Co (3)		H (3)		St (1)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I	S	I
V. H.....					5	6	2	3			2	9			9	18
High.....	29	10	8	15	18	23	43	35	24	10	16	24	27	24	165	141
M. H.....	13	16	17	10	12	1	6	1	3	9	13	6	12	16	76	59
Mod.....	9	15	12	18	21	5	5	3	6	5	17	5	29	8	99	59
M. Low.....	14	2	23	4			9				2		24	3	72	9
Low.....	13	2	25	5	14	10	21	4	3	3	21		15	4	112	28
Total.....	78	45	85	52	70	45	86	46	36	27	71	44	107	55	533	315

V. H. = Very High; M. H. = Moderately High; Mod. = Moderate; M. Low = Moderately Low.

In an early experiment, Su reported one case in which an image was of a low degree of vividness.

<sup>11</sup> With further training the O's confined their reports entirely to the lower part of the durative scale (cf. p. 75).



We find that the vividness of kinaesthetic sensations ranges from high to low. The sensations generally share their vividness with other members of the sensory complex, and are, therefore, not of the highest degree of vividness. The sensations in a perception of movement reach the highest degree of vividness only when they demand attention because of their quality (ache or strain), or their high intensity.

Most of the sensations reported by B, H, and St were "high" in vividness; most of those reported by Co and Z were "moderate;" and most of those reported by Ca and Su were "low." For Ca, Co, Su, and Z there is a pretty even distribution between the three rubrics of "high," "moderate," and "low."

The kinaesthetic image makes up in vividness what it lacks in the other attributes. Comparatively few images were reported "low" in vividness. In nearly all these cases, the lack of vividness was due to the occurrence of a visual image along with the kinaesthetic image. Co had more images of a low degree of vividness than any other O; she always reported, in such instances, "The kinaesthetic image was complicated by a visual image." Ca and B had also visual accompaniments to their kinaesthetic imagery, in which cases the vividness of the kinaesthesia varied from "moderate" to "low."<sup>12</sup>

The highest frequency occurred under the heading "high" for Co, H, St, Z, and Su; under "moderately high" for B; under "moderate" for Ca. When all the sensations reported are totalled, the highest frequency comes under the rubric "high," just as the total for all images shows as highest frequency the rubric "high."

The kinaesthetic image, then, is of a high degree of vividness. It has the advantage over sensation in its simplicity and uniformity; it has a chance to stand alone in consciousness for a brief time. While kinaesthetic sensations may be as often low in vividness as high, kinaesthetic images, unmixed with imagery from other sense departments, are never low.

#### (6) Locality

TABLE VI  
LOCALITY

	B (2)		Ca (3)		Co (3)		H (3)		Su (4)		Z (4)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Deep.....	25	10	50	7	9	3	56		77	30	83	18	300	68
M. Deep.....	7	5	18	10	57	12	21		11	2	41	33	155	62
M. Sup.....	12	3	8	10		2	46		9	4	2	1	77	20
Sup.....	16	14	6	28	16	29	5	45	30	2	55	25	128	143
Total.....	60	32	82	55	82	46	128	45	127	38	181	77	660	293
Def.....	23	16	41	34	10	29	13	35	54	13	61	8	202	135
M. Def.....	11	4	22	10	43	10	42	8	18	12	53	35	189	79
Indef.....	22	18	15	4	28	8	47		45	13	66	32	223	75
Total.....	56	38	78	48	81	47	102	43	117	38	180	75	614	289

M. Deep = Moderately Deep; M. Sup. = Moderately Superficial; Sup. = Superficial; Def. = Definite; M. Def. = Moderately Definite; Indef. = Indefinite. B reported 9 images which were localised "outside;" Su reported 19 images as "not localised."

<sup>12</sup> Where the visual image acted as a distraction, it perhaps would have been better to count the experiment unsuccessful. We have not done this, for the disturbance seemed to affect only the reports on vividness. We have, therefore, included everything reported by all our O's.

The depth of kinaesthetic sensation seems to be correlated with its quality. Dull pressure is deep or moderately deep; smooth pressure is deep or moderately deep; strain is more superficial; light pressure is superficial. The highest frequency of report for all *O*'s except Co was "deep;" the highest frequency for Co was "moderately deep."

The superficially localised sensations are, as a rule, definite in localisation; the deep sensations are sometimes quite definite, at other times indefinite. The highest frequency of report fell under the heading "definite" for B, Ca, and Su; under the heading "moderately definite" for Co; and under "indefinite" for H and Z: yet the highest frequency for depth, it will be remembered, came under "deep" or "moderately deep" for all these *O*'s, a fact which shows the difficulty of trying to correlate depth and definiteness. If we consider our total numbers, however, we find the highest frequencies coming under the rubrics "deep" and "indefinite."

The reports upon depth of the kinaesthetic image do not agree among the *O*'s. The highest frequency for B, Ca, Co, and H was "superficial;" for Z, "moderately deep;" for Su, "deep." Here no part of our scale for depth is favored by all *O*'s. B perceived some images as being outside the body, either just touching the skin or entirely separate from the body. Su reported 19 images which were so indefinite that she called them cases of "non-localization." They were unequivocal in reference; they *meant* that she was making the movement in imaginal terms, but they did not seem to be localized in the body.<sup>13</sup>

The images were most often definite in localization for Ca, Co, and H; indefinite for B; and moderately definite for Z. In the totals, we have our highest frequencies under "superficial"<sup>14</sup> and "definite."

Our study of locality reveals a greater diversity among *O*'s than we found in our study of the attributive characters. This is to be expected, for here we are dealing with perception. The image, perhaps because it was a surface-like feel, was perceived by most *O*'s as being at or near the surface. Yet the highest frequency of report for quality came under the heading of dull pressure for B, Ca, and Co, three of the four *O*'s whose highest frequency of report upon locality was "superficial." Again, Su's highest frequency for quality came under the heading of light pressure, while the locality of her images was most often deep. Again, we might be tempted to suppose that the image was localized at or near the surface because it was perceived, generally speaking, as flat and thin, like the cutaneous qualities. And yet Su and Z reported a great many images which were perceived as "flat," but were localized as deep or moderately deep. We can only say, then, that although quality and bidimensionality are probably conditions of the perception of depth in the kinaesthetic image, there are other important conditions which our reports did not reveal.

<sup>13</sup> There is an analogy to this indefiniteness of localization in the visual field. D. Katz characterizes the film-color as "indefinite" in localization; "Die Erscheinungsweisen der Farben und ihre Beeinflussung durch die individuelle Erfahrung," 1911, 73 ff.

<sup>14</sup> Superficial does not always mean "on the surface;" it may mean "skin-deep" or "just underneath the skin."



## (7) Pattern

TABLE VII

## PATTERN

	B (2)		Ca (2)		Co (3)		H (3)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Broad to narrow.....	8		3	3	1	2			14		2		28	5
Longer than broad.....	12		3	2	3	3	11		6		3		38	5
Spot to streak.....	2	15	3	2	21	19	8		6	23	18	7	58	66
Irreg. patch.....	3	3							12		7		22	3
Small spot.....				5	2	4		29	4	4	7	19	13	61
Qual. diff's.....			3		1				4		2		10	
Int. and viv. shadings.....	5	8	1		4	10			6	5	2		18	23
Total.....	30	26	13	12	32	38	19	29	52	32	41	26	187	163

Irreg. = Irregular; Qual. diff's = Qualitative differences; Int. & viv. shading = Intensive and vividness shading.

Su and Z reported also spots which became "rayed" on all sides, five times and once respectively; H reported a sharp, regular outline for the image 16 times; Z reported a "mosaic of spots" 7 times for sensation and twice for image (he could not decide whether it was qualitative or intensive.)

The totals in this table do not equal those in the former tables, because often there was no observable pattern in the kinaesthetic experience.

Kinaesthetic sensory complexes show great diversity of pattern. There were reported, by all observers but H, "masses" which ran out to points in pyramidal fashion; there were indefinitely outlined "patches," sometimes "bulky," sometimes "flat," and usually "longer than broad;" there were "spots or lumps" which stretched out to "bands" during their course; there were small spots which remained unchanged during their course, for all but B and Ca. Dimensionality did not come into all reports, but where it appeared, the kinaesthetic sensations are mostly tridimensional. The O's who did not report upon it were questioned at the end of the experiment, and stated that so far as they could remember most of their kinaesthetic sensations had been in three dimensions.

The qualities showed characteristic patterns. Strain was perceived as "stringy" or pointed; dull pressure as "massive" and "bulky," and often "elongated;" light pressure as "patchy," "sheet-like," or "flat." The patterns themselves had attributive differences within them: "nuclei" of dull pressure, strain, or smooth pressure, with "fringes" of light or bright pressure (for H and Z); "bands" of dull pressure with pointed ends of strain (for Ca, Co, and Su). Intensive and vivid variation, such as more intense or vivid center, point, or end, or "strips" or "cores" or "mosaics" of greater intensity or vividness, were reported by all.

The kinaesthetic images show more uniformity in pattern than the kinaesthetic sensations. Their pattern consisted almost entirely of small spots, which sometimes remained unchanged during their brief temporal course, and at other times ran out temporally to small streaks. Images, too, were bidimensional, or of very slight thickness.

For all O's the qualitative patterning of the images was uniform. For some O's (Ca and H) the images were uniform in all the attributes; for others there were at times intensive and vivid variations within the pattern: a less intense border which was perceived as

"fringed" if the quality was dull pressure; as "fuzzy," "feathery," or "like a bit of down, only smoother," if the quality was light pressure; a greater intensity or vividness at one end or side; or a wave of vividness which travelled over the image (for Ca and Su).

### (8) Temporal Course

TABLE VIII  
TEMPORAL COURSE

	B (4)		Ca (2)		Co (3)		H (4)		Su (3)		Z (3)		Total	
	S	I	S	I	S	I	S	I	S	I	S	I	S	I
Increase in intensity . . . . .	22	20	29	7	24	15	80		29	3	39		223	45
Increase in extent . . . . .	12	8	10	7	35	29	47		29	23	35	5	168	72
Increase in vividness . . . . .	18	8	13	8	6	1	5		10	11	3		55	28
Decrease in intensity . . . . .	31	17	18	2	2	2	14		8				71	21
Decrease in extent . . . . .	6		5	2	2		3						16	2
Decrease in vividness . . . . .	28			2	3				4		11		46	2
Addition of quality . . . . .	42		6		20		75		29		42		214	
Subtraction of quality . . . . .	37		7		6		31		7		16		104	
No change . . . . .		22		13			52		17		40			144
Total . . . . .	196	75	88	41	96	47	255	52	116	54	146	45	897	314

Kinaesthetic sensory complexes undergo attributive as well as processual changes in temporal course. All the O's reported increases in intensity, extent, and vividness, while some reported decreases in intensity, extent, and vividness. Processual changes were reported by all. A perception of movement might begin, *e. g.*, with dull pressure, to which would be added strain, and perhaps ache. The ache might be very brief, leaving the strain and dull pressure. Then the strain would drop out, perhaps, leaving the dull pressure. Addition of quality was more frequent than subtraction of quality for all O's except Ca.

Kinaesthetic images or imaginal complexes undergo very little attributive change and no processual change during their course. Often-times the image remained absolutely unchanged (for all O's but Co). Increase in some one of the intensive attributes was common with B, Ca, Co, and Su; most often in intensity for B and Ca, most often in extent for Co and Su. All of H's images were without pattern or temporal course; only 5 of Z's images had temporal course. Decreases in the intensive attributes were rarely reported by anyone but B. No processual changes were reported. An image never adds to itself a new process. It simply flashes in and out, with now and then a second image following it, but entirely separate from it.

### (9) 'Body' or Collocation of Attributes

Kinaesthetic sensations have more "body" than images. The intensive attributes in the sensory integrations are of varying degree. If a sensation is of very weak intensity, and of very limited extent, it will be, in all probability, somewhat long in duration. A haphazard search through reports on sensation shows the following combinations:

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Observer	Intensity	Extent	Duration
Co	weak	very extended	long
	strong	mod. limited	long
H	weak	mod. limited	mod. long
	mod. strong	extended	short
Su	weak	extended	mod. short
	weak	mod. limited	long
Z	weak	mod. extended	mod. short
	moderate	extended	long

The majority of reports on image for these 4 observers show integrations of this sort:

Intensity	Extent	Duration
weak	limited	short
mod. weak	very limited	very short
very weak	very limited	very short

Although there were reports of sensations which were weak, limited, and short, there were never reports of sensations as very weak, very limited, very short. Yet all *O*'s gave many reports of images which were very weak, very limited, and very short.<sup>15</sup>

## SUMMARY

Kinaesthetic sensation and kinaesthetic image compare thus:

(1) The qualities in kinaesthetic sensation are dull pressure, light pressure, bright pressure, smooth pressure, strain, and ache. The qualities in kinaesthetic image are all 'pressury,' dull pressure, light pressure, and possibly smooth pressure (cf. p. 73).

(2) The intensity of kinaesthetic sensation may be anywhere on the intensive scale from strong to weak. The intensity of the kinaesthetic image is always comparatively weak.

(3) Kinaesthetic sensations range all over the extensive scale from near zero to maximum, with a tendency however to group themselves about the upper part of the scale. The extent of the kinaesthetic image is for the most part limited, and it never reaches the maximum of extent attained by some sensations.

(4) The duration of kinaesthetic sensation is, in most cases, relatively long. The duration of the kinaesthetic image is

<sup>15</sup> Cf. J. Lindworsky, "Wahrnehmung und Vorstellung," *Zeits. f. Psych.*, LXXX, 1918, 215 ff.; C. Stumpf, "Empfindung und Vorstellung *Abh. d. kgl. preuss. Akad. d. Wiss.*, 1918. These articles reached us, unfortunately, too late for more than a mention at this place. See also E. B. Titchener, *Thought-Processes*, 1909, 20 f., 214; and for evidence of 'holding' in the sphere of sensation, R. T. Holland, *Journ. Exp. Psych.*, iii., 1920, 312 ff.

relatively short; it never reaches the highest part of the durative scale.

(5) The vividness of kinaesthetic sensations ranges from high to low. The vividness of the kinaesthetic image, on the other hand, is always high, unless the image is accompanied by imagery from other sense-departments.

(6) The locality of kinaesthetic sensations ranges from deep and indefinite to superficial and definite, with a tendency to be deep and indefinite. Dull pressure and smooth pressure are deep; strain and ache, more superficial; and light pressure, superficial. Kinaesthetic images are localized definitely by most *O*'s on or near the surface. They show no correlation between quality and depth or definiteness.

(7) Kinaesthetic sensory complexes show various types of pattern: 'pointed' or 'stringy' for strain; 'bulky' and 'roundish' or 'elongated' for dull pressure; and 'flat' and 'patchy' for light pressure. There are qualitative differences and intensive and vivid shadings within the sensory pattern. The sensations are for the most part tridimensional. Kinaesthetic imagery shows always the same pattern, a small spot, which sometimes increases temporally to a streak. It is uniform in all attributes for some *O*'s; it has intensive shadings for others. The images are most often bidimensional.

(8) The kinaesthetic sensory complexes show attributive as well as processual changes in temporal course, increasing and decreasing in all the intensive attributes, and changing in quality by addition and subtraction. Kinaesthetic imagery has very little, if any, temporal course, being generally too brief for change. Cases in which the image undergoes a slight increase in extent or intensity are the only exceptions to this statement.

(9) Kinaesthetic sensations have more 'body' than kinaesthetic images. What the sensation lacks in any one intensive attribute it makes up for in some other intensive attribute; the image may be poor in all the intensive attributes save vividness.

## II. RESIDENT AND PROJECTED KINAESTHETIC IMAGERY

We have now compared kinaesthetic memory-image with simple kinaesthetic perception, and have been able in this way to compare kinaesthetic sensation with the kinaesthetic simple image aroused under certain (*i.e.*, memory) conditions. We have, however, no reason to suppose that we have exhaustively described 'the' kinaesthetic simple image. Under other conditions of arousal, the image may show in other

phenomenal modes. In particular, there may (we thought) be various attributive differences between the resident kinaesthetic image of memory and the projected kinaesthetic image of empathy, if indeed we have any psychological right to speak of a 'projection' of kinaesthesia at all. This, then, is the point upon which our further experimentation is directed.

We made out a new list of paired commands for the arousal of kinaesthetic imagery, half of them representing situations which called for usual, ordinary, familiar movements, similar to those of the preceding experiments, the other half representing situations in which there were unusual movements.

The experimental procedure was the same as before. The instructions, too, were the same. The work began at the end of October, and continued throughout the next four months. Two of our O's, Ca and St, left the university; the remaining five served in this second period of our work.

#### *Experiment III*

We performed about 50 preliminary experiments, in order that our O's might attain a fair degree of stability of attitude. We found that now, when the situations were more complex, the visually minded O's realized the situations for the most part in visual terms, in spite of the instructions. We decided, therefore, to change the wording of our commands, making them read, "Feel: Tantalus standing in water, trying in vain to drink," instead of "Think of: Tantalus standing in water, etc." We explained that by "feel" we meant "realize kinaesthetically." This slight change in the stimuli effected wonders in the results. Visual imagery became unimportant, while the kinaesthetic imagery, in many cases, carried the whole meaning of the situation.

#### *Experiment IV*

We were now ready to begin the main part of the experiment. We made out fifteen paired commands, one member of every pair referring to a usual situation, the other member referring to an unusual situation.<sup>16</sup> Again we fractionated the characters to be reported upon, asking in Group I for quality, intensity, and extent; in Group II. for duration, vividness, and locality; in Group III. for pattern and temporal course. We repeated every group, so that we had 60 experiments for every group.

<sup>16</sup> Examples of the stimuli are: "Feel: yourself running downstairs." "Feel: an acrobat walking a tight rope." "Feel: Laocoon struggling in the coils of the serpent." "Feel: yourself stooping to pick up a pencil." The colon after the word "feel" indicates that a short pause was made after this word in reading the stimuli.



### Results

We found that the kinaesthetic images aroused by our stimuli may be classified under two main headings: (1) resident kinaesthesia, which is like that studied in the first part of the paper, the kinaesthetic imagery which ordinarily is referred to oneself; and (2) projected kinaesthesia, the kinaesthetic imagery which is projected outward, and is referred ordinarily to someone else or something else.

The *O's* came upon the projection gradually. *H. e. g.*, said: "That kinaesthesia was not in me; it belonged to the visual image." "The kinaesthesia did not relate to myself; it related to someone else." And later (of an image aroused by the stimulus, "Feel: a farmerette pitching hay"), "The kinaesthetic image belongs to the farmerette, it is a kind of empathy."

*Z.*, after reporting for some time the fact that some of his kinaesthetic images did not belong to himself, but to the person referred to in the stimulus, said: "It is a kind of projection."

*Su* reported at first thus: "The kinaesthetic image belonged to myself; then it was projected into an imaginary figure." And for another situation, relating to Tantalus trying in vain to drink: "The imagery was in me and then in Tantalus, but somehow it was transferred from the right side of my neck to the left side of Tantalus, who was out in front and facing me." Later, *Su* reported: "It seems as if the kinaesthesia is lifted bodily out of me and put into someone else; then it changes attributively. Sometimes, it sinks back into me at the end of the report." Still later *Su* reported projection without having first noted the beginning in resident kinaesthesia.

*Co* reported for the stimuli referring to the self: "The kinaesthetic image was in me," but for the stimuli referring to someone else: "The kinaesthetic image was not in me, but went with the visual image to make up the situation."<sup>17</sup>

Finally, *B* said of a projected kinaesthetic image: "It is as if it were fully formed out in space, and happened to catch my attention." And again: "It is fully formed like an object out there to be looked at."

Thus, all the *O's* came, rather slowly but certainly, to a realization that some of their images were resident, while others were projected out into a foreign object.

We found, however, that before the resident and projected kinaesthesia could be differentiated clearly, we had to break up an attitude which some of the *O's* had carried over from the work on sensation and image. There, it will be remembered, because of the mistakes of several *O's* who "held" or "built up" their images, we had warned all the *O's* against a shift from the passive or receptive attitude towards sensation to an active or constructive attitude toward their imagery. When their attention was thus called to the change, they realized that the images which were taken passively, as the sensations were, did not persist, but were sudden and brief. It was then natural to keep the passive attitude, and to be content to report upon the brief "flash" of kinaesthesia as it came and passed.

<sup>17</sup> Two of the *O's* always had visual accompaniments to their projected kinaesthesia; three had nothing but kinaesthetic imagery, the whole imaginal figure and the meaning of the situation being carried in terms of dull pressures, light pressures, bright pressures, strains and aches.

But here where we are working upon a differentiation within the realm of imagery itself, the *O's must be able to shift attitudes*. The projected kinaesthesia begins as a brief flash of resident kinaesthesia, which is then projected out into a foreign object, and becomes stabilized as the attitude shifts from passive to active. After an *O* has become habituated to the active attitude of projected kinaesthesia, the beginning bit of resident kinaesthesia may fail to be observed. Before such habituation, however, the resident kinaesthesia and the projected kinaesthesia may alternate, making the experience mixed. If, now, the *O* is accustomed to the passive attitude, he will seize upon the fleeting resident image, and will begin his report immediately after this 'flash' of experience, instead of waiting for 'stabilization.' He will thus report upon resident kinaesthesia partially changed, perhaps, by admixture of a bit of projected kinaesthesia.<sup>18</sup>

As soon as the *O's* became familiar with the two attitudes, we asked them to alternate the two. When we instructed them to take an active attitude toward the stimuli which normally gave resident kinaesthesia, they reported that the image could not be made to persist, but slipped away, repeated itself, and produced a quick succession of images of like patterns. This shows that true 'stabilization' was not possible. When we instructed the *O's* to take a passive attitude toward the stimuli which normally gave projected kinaesthesia, they reported different patterns with each succeeding 'flash' of experience, until in the end all the separate patterns returned, became stabilized, and persisted as a complete situational image. This shows that stability was necessary here for a complete realization of the experience. In these cases, neither the 'flash' nor the 'stabilization' was quite natural: the 'flash' was not the totally involuntary thing that normally occurs in resident kinaesthesia, and was longer in time; and the 'stabilization' was not complete as in projected kinaesthesia; there were gaps in time, in which there was no process. The *O's* thus discovered that, although either attitude is possible with any stimulus, the active attitude is natural for projected kinaesthesia, as the passive attitude is natural for resident kinaesthesia. They reported also that the active attitude seems to grow naturally out of the passive, as stability grows naturally out of a beginning 'flash.'<sup>19</sup>

From the statements of the *O's* in characterizing the two attitudes, we have gleaned the following differences between the two. The attitude in projected kinaesthesia is the perceptive attitude; the situation is objectified, and the *O* contemplates it as he would a landscape; the report upon the imagery is more like an *Aussage*-report than a scientific description; the attitude is correlated with imagery which is so full that it is not all clear at once, imagery which is persistent and unchanging as the different parts become vivid in turn for report; the attitude is active, exploratory, detached. The attitude in resident kinaesthesia is the sensory attitude, the same as that of the earlier part of our work; the *O* experiences the imagery as he would a sensation or an after-image; the report is the ordinary laboratory

<sup>18</sup> The two terms 'flash' and 'stabilization' were used by an *O* in distinguishing the experiences under the two attitudes.

<sup>19</sup> It may be that some of Perky's critics have been misled by failure to recognize the different attitudes. Their *O's* may not have waited for stability, but may have reported upon the first brief 'flash' of experience; and may then have called this an 'image of imagination,' when in reality it was a 'memory image' from which an 'image of imagination' might (with shift of attitude) have developed.

description: the attitude is correlated with imagery which is all clear at once, and is fleeting and changing; the attitude is passive, receptive, realizing.

#### A. RESIDENT AND PROJECTED KINAESTHETIC IMAGERY COMPARED

The following tables, arranged upon the same plan as those for the comparison of sensation and image, show the difference in the psychological pictures of resident (R) and projected (P) kinaesthesia.

##### (1) *Quality*

TABLE IX

##### QUALITY

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Du. P	30	29	30	26	85	1	22	9	41	18	208	83
Brt. P	11				34		18		1		64	
Drag					2				2		4	
Sm. P					13		5		1		19	
Strain	2		16		45		3		10		76	
Ache	1		4		21		2		6		34	
Lt. P	1	26	4	7	16	43	32	32	26	11	66	119
Total	45	55	54	33	216	44	82	41	87	29	471	202

P = projected kinaesthetic imagery; R = resident kinaesthetic imagery.

Du. P. = Dull Pressure; Brt. P. = Bright Pressure; Sm. P. = Smooth Pressure.

Lt. P. = Light Pressure (or Neutral Pressure.)

Projected kinaesthetic imagery is qualitatively very much like sensory kinaesthesia. Dull pressure, strain, ache, and light pressure are common. Bright pressure occurs much more often in the projected imagery than in the sensory complexes; only one *O* failed to report it here, while only two *O*'s found it in actual sensations.<sup>20</sup> The existence of smooth pressure in projected kinaesthesia is doubtful; only three *O*'s reported it, and that in a comparatively small number of cases. Drag is not common in projected kinaesthesia.

From 4 to 7 different qualities were reported in projected kinaesthetic imagery. Dull pressure has the highest frequency in the average, and for the individual *O*'s with the exception of Su.

Resident kinaesthetic imagery has but two qualities, dull pressure and light pressure. In the preceding part of our work we were in doubt whether to include smooth pressure in the list of qualities for

<sup>20</sup> A 'textural' difference is reported between the bright pressure of sensation and the bright pressure of projected kinaesthesia. In sensation, it is 'sparkling' like a liquid upon which a light is shining; in projected imagery, it is misty, like a halo or a sun-lighted cloud.



imagery. Since none of the *O*'s, after further training, reported it, we may conclude that it is not characteristic of resident kinaesthetic imagery (cf. p. 59). Light pressure has the highest frequency in the average, although individual *O*'s gave the highest frequency to dull pressure.

Here, where our situations are more complex, a single image-process did not always stand for the whole situation. Often the reports showed an imaginal complex made up of a number of simple processes following one another in quick succession, and differing from one another very slightly. There were never two such processes present at once, as there were in the projected kinaesthesia and in sensory kinaesthesia.

## (2) *Intensity*

TABLE X

## INTENSITY

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Strong.....	14		17		33		8				72	
M. St.....					23		5		5		33	
Mod.....	5	19	25	12	33		21	3	32	16	116	50
M. Wk.....	5	7			5	2	11	12	37	15	58	36
Weak.....	10	16	8	17	26	29	27	11	17	3	88	76
V. Wk.....	8	13	4	4	14	10	14	14	5		45	41
Total.....	42	55	54	33	134	41	86	40	96	44	412	203

M. St. = Moderately Strong; Mod. = Moderate; M. Wk. = Moderately Weak; V. Wk. = Very Weak.

The intensity of projected kinaesthetic imagery ranges from strong to weak, with the highest frequency of the average coming under 'moderate' as in the sensory complexes. The highest frequency for B falls under the rubric 'strong;' for Co, under 'moderate;' for Su, under 'moderately weak.'

The intensity of resident kinaesthetic imagery ranges from moderate to very weak, with the highest frequency averaging 'weak.' The highest frequency for B and Z occurs under 'moderate;' for Co and H, under 'weak;' for Su, under 'very weak.'<sup>21</sup>

<sup>21</sup> We see that the *O*'s have ceased to report any resident kinaesthetic images in the upper half of the intensive scale (cf. p. 60). Training undoubtedly had an effect here; but there is a more important factor to be considered, namely, the fact that the *O*'s had no absolute criterion. It was natural, then, that the resident imagery should be comparatively weaker when compared with projected kinaesthesia, which represented large and complicated movements, than it was when compared with simple sensory movements, such as we used for stimuli in the earlier experiments.

(3) *Extent*TABLE XI  
EXTENT

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
V. Ex. ....	2				33		6				41	
Ext. ....	3		7		22		14		1		47	
M. Ex. ....	20				29		25		3		77	
Mod. ....	2	16	31	7			11	3	39	6	83	32
M. Lm. ....	4	11			13		12	7	33	11	62	29
Lim. ....	5	10	16	30	6	9	15	16	12	12	54	77
V. Lm. ....	6	16	1		4	21	3	14	10	5	24	56
Total .....	42	53	55	37	107	30	86	40	98	34	388	194

V. Ex. = Very Extended; Ext. = Extended; M. Ex. = Moderately Extended; Mod. = Moderate; M. Lm. = Moderately Limited; Lim. = Limited; V. Lm. = Very Limited.

Projected kinaesthesia, like sensory kinaesthesia, varies in extent from very extended to very limited. The highest frequency comes under the heading 'moderately extended' for B and Su; under the rubric 'moderate' for Co and Z; under the rubric 'very extended' for H.

According to the table, the resident kinaesthetic image never goes beyond the middle part of the extensive scale.<sup>22</sup>

The highest frequency of report is 'limited' for Co, Su, and Z; and 'very limited' for H. The highest frequency in the totals comes under the heading 'moderate' for projected kinaesthetic imagery; the highest frequency in the totals comes under the heading 'limited' for resident kinaesthesia. Thus all the resident images are found in the lower half of the scale, while over 50 per cent of the projected images are found in the upper half of the scale of extent.

(4) *Duration*

TABLE XII

## DURATION

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Long .....	32		48		175		92		117		420	
M. Lg. ....												
Mod. ....				1								1
M. Sh. ....		13		4			8		6		28	
Short .....		6		24		1	11		5		42	
V. Sh. ....		17		9		49	14		19		90	
Total .....	32	36	48	38	175	50	92	33	117	30	420	170

M. Lg. = Moderately Long; Mod. = Moderate; M. Sh. = Moderately Short; V. Sh. = Very Short.

<sup>22</sup> Training plus a larger relative scale for comparison has again levelled out the discrepancies which were found in the earlier reports (cf. p. 61).

Projected kinaesthetic images are always long in duration. The whole image comes fully formed, and there is no addition or subtraction of process.

Resident kinaesthetic images are always short in duration. The highest frequency for all O's falls under the rubrics 'short' and 'very short.' The highest frequency in the totals comes under the rubric 'very short.'<sup>23</sup>

### (5) *Vividness*

TABLE XIII

## VIVIDNESS

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
V. H. ....	6	4	1	7	20	43	2	4	1	15	30	73
High .....	9	9	19	14	56	6	25	16	2	10	111	55
M. H. ....	13	19	12	11	7		33	8	20	3	85	41
Mod. ....	5	5	8	1	6		19	5	33	2	71	13
M. Low .....	2				30				30		62	
Low .....			6	1	60		13		33		112	1
Total .....	35	37	46	34	179	49	92	33	119	30	471	183

V. H. = Very High; M. H. = Moderately High; Mod. = Moderate; M. Low = Moderately Low.

The vividness of projected kinaesthesia is divided evenly between high and low. Not all of the projected experience can be in the focus of attention at one time. The highest frequency of report falls under the heading 'moderately high' for B and Su; under 'high' for Co; and under 'low' for H.

The vividness of resident kinaesthesia ranges from very high to moderate. Most of B's images were 'moderately high;' most of Co's and Su's were 'high;' most of H's and Z's were 'very high.' The greatest number of images in the total falls under the heading 'very high.'

<sup>23</sup> The table for duration shows markedly the relativity of the subjective scale. In the earlier experiments, most of the images were 'short' in duration, but here by far the greatest number of images are 'very short' in duration (cf. p. 62). We can account for the change in report by the fact that the projected imagery is (objectively) very long in duration.

(6) *Locality*

TABLE XIV

## LOCALITY

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Deep.....	17	10	13		85		21	8	37	1	173	19
M. Deep.....	8	9	31		53		16	4	56	3	164	16
M. Sup.....	3	3	1	15	38	5	21	6	18	10	81	39
Sup.....	7	8	2	19			44	32	9	16	50	92
Total.....	35	30	47	34	176	49	90	23	120	30	468	166
Def.....	8	23	9	29	65	46	23	5	3	19	108	122
M. Def.....	10	8	21	4	41	6	24	7	41	9	137	34
M. Ind.....		1			23		4	11	38	1	65	13
Indef.....	15	3	18	1	49		39		38	1	159	5
Total.....	33	35	48	34	178	52	90	23	120	30	469	174

M. Deep = Moderately Deep; M. Sup. = Moderately Superficial; Sup. = Superficial; Def. = Definite; M. Def. = Moderately Definite; M. Indef. = Moderately Indefinite; Indef. = Indefinite.

B reported 2 sensations and 5 images localized "outside;" Su reported 9 images as "not localized."

Generally speaking, projected imaginal kinaesthesia, like sensory kinaesthesia, is localized as deep and indefinite. There is, however, much individual variation. As to depth, the highest frequency for B and H is under the rubric 'deep,' for Co and Z, under 'moderately deep;' for Su, under 'superficial.' As to definiteness, the highest frequency for B and Su comes under the rubric 'indefinite;' for Co and Z, under 'moderately definite;' for H, under 'definite.'

Resident kinaesthetic images are most often localized as superficial and definite. Most of Co's, H's, and Z's images were 'superficial' and 'definite;' most of B's were 'deep' and 'definite;' most of Su's were 'deep' and 'moderately indefinite.'

We find very few images in resident kinaesthesia which are indefinite in locality, although earlier (p. 63) we had a fair proportion of images reported under this rubric. We know of no way to account for the change except by the effect of training. One O said of resident kinaesthetic images: "They are so definitely localized that I could pin a ribbon on them."

We find, as before, cases of non-localization of resident images.

(7) *Pattern*

The pattern of projected kinaesthesia is even more varied than that of sensory kinaesthesia. All O's reported 'long strips' of bright pressure or dull pressure; 'strings' of strain; masses, longer than broad, of dull pressure; irregular patches of light pressure or bright pressure; small spots of dull pressure or light pressure. The bright pressure was sometimes characterized as 'hollow,' like a short tube

TABLE XV

## PATTERN

	B (2)		Co (2)		H (2)		Su (2)		Z (2)		Total	
	P	R	P	R	P	R	P	R	P	R	P	R
Strip.....	5	12	3	24	7		4	16	24	16	43	68
Longer than broad.....	27		18		92		31	6			168	6
Irreg. Patch.....	3		5		5		43		10		66	
Spot.....		9		7	50	52	28	16	51	11	129	95
Qual. diff's.....			13		104						117	
Int. and Viv. shadings.....	4	11	22	16	121		49	16	96	7	292	50
Def. outline.....				4		52		9			9	56
Indef. outline.....	1		14				6				21	
Total.....	40	32	75	51	379	104	170	54	181	34	845	275

Irreg. patch = Irregular patch; Qual. Diff's = Qualitative differences; Int. and Viv. shadings = Intensive and vivid shadings; Def. outline = Definite outline; Indef outline = Indefinite outline.

The following were also reported: "hollow pattern" for Projected Kinaesthesia, 10 (Su), 1 (B); "mosaic of spots" for Projected Kinaesthesia, 20 (Z).

or 'band-box.' This is a pattern which did not occur in sensory kinaesthesia. As in sensory kinaesthesia, so in projected kinaesthesia there were qualitative differences within the patterns, as well as intensive and vivid shadings, characterized as 'striped,' 'mosaics,' 'wrinkled,' 'spotted,' or 'fringed.' The projected patterns were often very indefinite in outline. They were both tridimensional and bi-dimensional. It is noteworthy that the frequency of report under every heading is much larger for the projected kinaesthesia than it was for the sensory kinaesthesia (p. 65). It is obvious that this difference is to be explained by the fact that the patterns in projected kinaesthesia persist unchanged while the *O* explores them, whereas the patterns in sensory kinaesthesia are in constant flux, and must be caught 'on the wing' for report.

The resident kinaesthetic images, like the kinaesthetic images of the earlier experiments, are characterized generally as small spots, which sometimes remain unchanged and at other times run out temporarily to streaks. Again we had no qualitative differences reported, but quite often intensive and vivid differences, such as "fringes" or "fuzzy" borders. As opposed to the projected kinaesthesia, the resident kinaesthesia is definitely outlined and, for the most part, bi-dimensional.

### (8) Temporal Course

Projected kinaesthetic imagery shows no change in time. The imagined situation, made up entirely of kinaesthetic imagery, or supplemented by visual imagery, remains fully formed out in space, like an object at which we are looking, so that the *O* reports from the actual (imaginal) perception.

Resident kinaesthetic imagery comes more suddenly than projected imagery, and remains only a brief time, a time too brief to allow of any change for some *O*'s, but long enough for others to permit a slight increase in intensity or extent. The resident kinaesthetic images pass so quickly that the *O* must report them from memory.

## SUMMARY

Projected and resident kinaesthetic imagery compare thus:

(1) The qualities in projected kinaesthesia are dull pressure, light pressure, bright pressure, strain, and ache. The qualities in resident kinaesthetic imagery are dull pressure and light pressure.

(2) The intensity of projected kinaesthesia ranges from strong to weak. The intensity of resident kinaesthesia ranges from moderate to weak.

(3) The extent of projected kinaesthesia ranges from maximum to near zero. The extent of resident kinaesthesia ranges from moderate to near zero.

(4) The duration of projected kinaesthesia is always long. The duration of resident kinaesthesia is always short.

(5) The vividness of projected kinaesthesia ranges from high to low. The vividness of resident kinaesthesia ranges from very high to moderate.

(6) The locality of projected kinaesthesia varies from deep and indefinite to superficial and definite, with a tendency toward depth and indefiniteness. The locality of resident kinaesthesia tends to be superficial and definite.

(7) Projected kinaesthesia shows various types of pattern: 'strips,' 'masses,' 'patches' and 'spots.' These patterns have qualitative differences and shadings of intensity and vividness. Both two and three dimensions are common. The resident kinaesthesia is unvaried in pattern, being ordinarily a spot which may run out temporarily to a streak. It sometimes has slight shadings of intensity or vividness. It is most often bidimensional.

(8) Projected kinaesthesia shows no change throughout its long temporal course. Resident kinaesthesia may show slight attributive changes during its brief temporal course; it never shows processual changes.<sup>20</sup>

### 3. Some Conditions of Resident and Projected Kinaesthetic Imagery

In the foregoing experiment we obtained resident kinaesthetic imagery when the stimuli represented ordinary situations, such as struggling to pick up a pencil. But these stimuli always referred to the self: "Feel yourself struggling to pick up a pencil." Further, we obtained projected kinaesthetic imagery when the stimuli represented unusual situations and movements not likely to have been made by the self, such as walking a tight rope. But these stimuli referred always to someone else: "Feel as another walking a tight rope."

<sup>20</sup> Numerous exceptions which are usually referred to association, as when we say a thing looks cold or hard, may very well be, in the light of these results, impressions at the level of perception. But we know hardly anything of kinaesthetic imagery.



We asked ourselves, then, which is the stronger influence upon resident and projected kinaesthesia, the influence of the usual and unusual, or the influence of the self and another? The natural thing to do was to reverse the situations, so that the usual should refer to another person, the unusual to oneself. We made out new paired commands which fulfilled this requirement.

We found that one of three things happened. (1) As a rule, the *O*'s reported resident kinaesthesia with the stimuli referring to the self, and projected kinaesthesia with the stimuli referring to the other person, in spite of the usualness or unusualness of the situation. (2) Sometimes the report was mixed, resident kinaesthetic imagery occurring for part and projected for part of the same situation. This mixture occurred with stimuli referring to the self making some strange movement. The thought of the movement would begin in resident kinaesthetic imagery, but the imagery would quite suddenly project itself without changing the meaning in any way except to make the self-reference uncertain. The *O* would say: "I think that was myself in the projected imagery, but I am not at all sure; it was a detached self." Every *O* who experienced this projected kinaesthesia in the midst of resident said: "I wonder if the projection happened there because I have never performed such a movement." Thus H, under the instruction "Feel: yourself making a hundred yard leap on a pair of skis," experienced the beginning of the leap in resident kinaesthesia, the movement through the air in projected kinaesthesia, and the landing in resident imagery. He said: "I have never leaped on a pair of skis, but I have made leaps, and that may be the reason that I had resident for the beginning and end of the leap." (3) Sometimes pure projected kinaesthesia resulted when the stimulus referred to the self performing a very strange feat.

We now had the reversal from resident to projected kinaesthesia for certain situations referring to the self in a strange situation; but we had not the corresponding reversal from projected to resident for usual situations referring to someone else. We thought that the failure to obtain the second reversal might be due to the fact that our stimuli for these experiments did not equal in familiarity the strangeness of the stimuli for the first reversal; i. e., the *O*'s were *shocked* into projection by the strangeness of the one, and they must be *shocked* into resident imagery by the familiarity of the other. We therefore changed our situations, making them refer to someone else figuring in situations in which we had seen our *O*'s figure only a few days before. Immediately, and to the immense surprise of the *O*'s themselves, who had come to expect projection for all situations referring to other persons, resident kinaesthesia resulted.

We found, then, that the *O*'s could be shocked into projection in spite of the self-determination, and could be shocked into resident kinaesthesia in spite of the other-determination. The conditions thus cut across each other; ordinary usualness-unusualness is less strong a determinant than selfness-otherness, but a salient familiarity-strangeness, coming to the *O* with a shock of surprise, may reverse the normal conditioning of the two modes of imagery. It is plain that we are here upon the threshold of a highly complex problem, whose resolution will require methods more refined than those which we had at our disposal in the present study.

## CONCLUSION

(1) Kinaesthetic images of memory may be distinguished from kinaesthetic sensations by uniformity, simplicity, and lack of 'body.' Kinaesthetic memory-images are uniform: always tiny bits of pressure, absolutely lacking in brightness. They are simple: they approximate single processes, and take on few or no perceptive characters. They lack 'body:' they are low in all the intensive attributes save vividness. Kinaesthetic sensations in a perception of movement are varied, complex, and have 'body:' they are now of one quality, now of another: they are sometimes high in all intensive attributes, sometimes high in only one, but always have more 'body' than the image: they constitute a sensory complex rich in all the perceptive characters.

(2) Kinaesthetic imagery may be classified under two main headings, resident and projected. The resident kinaesthetic images are referred ordinarily to oneself: the projected kinaesthetic images are referred ordinarily to someone else or something else. Resident kinaesthetic images may approximate simple processes, co-ordinate with sensation: projected kinaesthetic images constitute imaginal complexes, co-ordinate with perception. Resident kinaesthetic images show a psychological picture very different from the picture of kinaesthetic sensations in sensory complexes: projected kinaesthetic images in an imaginal complex show a psychological picture very like the picture of kinaesthetic sensations in a sensory complex. Resident kinaesthetic images are changing, fleeting, and involve an attitude which is passive, receptive, realizing: projected kinaesthetic images are invariable and persistent, and involve an attitude which is active, exploratory, detached, scrutinising.

(3) The difference between resident and projected kinaesthesia does not reflect merely the functional distinction of self and other: it is correlated with a specific difference of attitude on the part of the observer.



# AFFECTIVE TENDENCY AS CONDITIONED BY COLOR AND FORM<sup>1</sup>

By MATSUSABURO YOKOYAMA

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## INTRODUCTION

This investigation studies the simultaneous effect of two aspects of a simple sensory material in conditioning affective tendency, and the nature of the affective resultant. The

<sup>1</sup>From the Psychological Laboratory of Clark University. The writer wishes to acknowledge his indebtedness to Dr. L. R. Geissler for suggestions in the initiation of this research.

The present article has been condensed for purposes of publication by the omission of numerous tables that present special details or represent intermediate phases of the quantitative argument and by the abbreviation or elimination of much of the discussion. A complete bound manuscript, which gives the original data, the full numerical account and a complete discussion is available under this title in the Clark University Library and can be borrowed under the usual courtesies of library exchange.

problem in its broader phase—in its bearing upon the question of summation of affections—is not new. Wundt,<sup>2</sup> in his analysis of affective fusion, declares that the characteristic of feeling lies in the fact that all feelings present in consciousness at a given moment tend to fuse into a unitary manifold, a *Totalgefühl*, which, nevertheless, is not a mere sum of its components, but has a unique property of its own. Külpe,<sup>3</sup> MacDougall,<sup>4</sup> and Titchener,<sup>5</sup> on the other hand, hold that the affection of any given moment is the algebraical sum of the affections attaching to all the various sensory processes that constitute the mind at that moment.

Unfortunately, however, there seems, with a single exception,<sup>6</sup> to have been no systematic experimentation in support of either of these views. The question occurs, it is true, in various studies of color-harmony and of mixed feelings;<sup>7</sup> but these are too few in number and of too fragmentary a nature to warrant general psychological conclusions. The problem, as it stands to-day, like other problems in affective psychology, is still in controversy, pending solution by experiment.

#### PROCEDURE

The present work was conducted in the psychological laboratory of Clark University during the academic year 1919-1920.

*Observers.*—The observers in the experiment were B, Dr. E. G. Boring, who worked with knowledge of the problem; D, Dr. L. D. Boring; F, Dr. S. W. Fernberger; M, Miss M. Bates; P, Mr. C. C. Pratt. All but M were highly trained observers and had had experience in observation under the conditions of the experiment. M soon acquired introspective facility.

<sup>2</sup> W. Wundt, *Feelingpsychologie*, *Psychologie*, 2 Aufl. 1910, II, p. 351 ff.

<sup>3</sup> O. Külpe, *Outlines of Psychology*, trans. 3 ed. 1909, p. 264.

<sup>4</sup> W. MacDougall, *Psychological Psychology*, 1905, p. 80; *Body and Mind* 3 ed. 1915, p. 333.

<sup>5</sup> E. B. Titchener, *Text-book of Psychology*, 1910, p. 258.

<sup>6</sup> L. R. Geissler, The Affective Tone of Color-Combinations, *Titchener Chromaticity Tables*, 1917, 154-174. Geissler found that the pleasantness of color-pairs increases directly with the pleasantness of the colors taken individually.

<sup>7</sup> Thomas Cohen, Experimentelle Untersuchungen über die Gefühlsbestimmung der Farben, Helligkeiten und ihrer Combinationen, *Philos. Stud.* 11, 1904, 50-60; C. H. Johnson, The Combination of Feelings, *Harvard Psychol. Stud.* 1, 1910, 154-161; C. E. Kellogg, Alternation and Interference of Feelings, *Psychol. Mon.* No. 79, 1905, 85 pp.; A. Wagemann, Pleasure-Displeasure, *See J. Psychol. Mon. Suppl.* No. 1, 1910, 152 pp.

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**Stimuli.**—The stimuli, except in Group II, were colored forms cut from the Milton-Bradley pigment papers and pasted on squares of neutral-gray cardboard, 14.5 x 17.5 cm. The list of stimuli and their designations follows:

## Group I.

- Rs: red square, 5 x 5 cm.
- Os: orange square, 5 x 5 cm.
- Ys: yellow square, 5 x 5 cm.
- Gs: green square, 5 x 5 cm.
- Cs: cyan (bluegreen) square, 5 x 5 cm.
- Bs: blue square, 5 x 5 cm.
- Ps: purple (redviolet) square, 5 x 5 cm.

## Group II. Each of the following forms is drawn in black ink on a neutral-gray cardboard, 14.5 x 17.5 cm. The thickness of the line is about 0.5 mm.

- Na: equilateral triangle, 7 cm. to a side, resting on its base.
- Nb: circle, radius 2.8 cm.
- Nc: annular sector, radius of larger arc, 7.3 cm.; of smaller arc, 2.5 cm.; placed convex upward; the two sides subtend an angle of 60 degrees.
- Nd: regular pentagon, 3.8 cm. to a side, resting on one of its angles.
- Ne: regular hexagon, 3.2 cm. to a side, resting on one of its angles.
- Nf: rectangle, 6 x 4.15 cm., resting on its shorter side.
- Ng: ellipse, major axis 3.5 cm.; minor axis 2.35 cm.; major axis horizontal.

## Group III. 49 colored forms, i. e., every color of Group I, combined with every form of Group II. Capitals designate the color (cf. Group II). Thus,

- |                   |                      |
|-------------------|----------------------|
| Ra, red triangle  | Yd, yellow pentagon  |
| Rb, red circle    | Cf, cyan rectangle   |
| Oc, orange sector | Pf, purple rectangle |

The brightness of the colors and the gray cardboard (N) was determined by flicker photometry under the artificial daylight of a frosted 75-watt type C-2 Mazda lamp, 15 cm. distant. The average determinations for B and the writer, reading in both directions, and expressed in per cent of baryta white mixed with velvet black paper, are:

## Colored

paper	R	O	Y	G	C	B	P	N
White	11.7	32.3	91.2	46.5	39.5	12.9	19.5	38.1

**Method and apparatus.**—The method of paired comparisons was used throughout the entire course of experimentation.

The inapplicability of the method of choice for investigating the affective value of color has been shown by Cohn.<sup>8</sup> The value of the method of single exposures (*Reismethode* after M. Brahn) has been questioned by Nakashima.<sup>9</sup> While the serial method (method of isolated exposures) has been highly recommended by Major,<sup>10</sup> it is

<sup>8</sup> Cohn, *op. cit.*, 564.

<sup>9</sup> T. Nakashima, *Amer. J. Psychol.*, 20, 1909, 180.

<sup>10</sup> D. R. Major, *Amer. J. Psychol.*, 7, 1895, 59.

not practicable in our research because of the non-serial nature of our stimuli. The method of paired comparisons was chosen, in spite of the objections of Gordon,<sup>11</sup> and Bullough<sup>12</sup> since it is the method which has been applied by Titchener,<sup>13</sup> Hayes,<sup>14</sup> Cohn,<sup>15</sup> Geissler,<sup>16</sup> and others with some degree of success in the investigation of affective problems.

Recently Barrett,<sup>17</sup> having tested experimentally the relative value of the order of merit method and the method of paired comparisons, came to the conclusion that the former is "vastly to be preferred to" the latter "from the standpoint of their relative demand upon the time and energy both of the experimenter and of the subjects." This conclusion, to be sure, is legitimate as far as it goes, but the use of the order of merit method is less desirable where scientific thoroughness is required. We tried this method for testing the preference of colors and forms on over 200 students of Wellesley and Clark Colleges, and found it quite unsatisfactory because of the observers' inability to comprehend the large number of stimuli, which inevitably led to various memorial confusions, as well as to neglect of areas beyond the visual range, and because of the tendency to make mediate judgments based upon reflection or upon previously established choices, i. e., the tendency to be "logical." In addition to these defects, there were spatial errors, contrast effects, extraneous associations, etc.

A neutral gray cardboard screen, 120 x 57 cm. on a wooden framework was erected on a table 120 x 60 cm., 76 cm. high. In the center of the cardboard were two square openings, each 10.5 x 10.5 cm., placed side by side at a distance of 9.5 cm. The table on the side towards O was covered with neutral gray cloth which hung to the floor. On E's side of the screen an oblong board, 43 x 16 cm., was hinged to the wooden frame, 19 cm. above the table, in such a manner that it could be let down towards E and two stimulus cards arranged on it side by side with each card held in place by a triangular piece of tin into which one corner slipped. When this board was snapped up by a rubber band into the upright position the stimuli came directly behind the square openings. Hinged at the top of the screen and falling toward O was a neutral gray shutter, 45 x 27 cm., which completely covered the square openings. A string was attached to the shutter from behind so that it could be lifted by E, disclosing the stimulus without letting shadow fall upon it. A vernier chronoscope<sup>18</sup> was used for taking reaction times. It was

<sup>11</sup> K. Gordon, *Psychol. Rev.*, 19, 1912, 354.

<sup>12</sup> L. E. Bullough, *Brit. J. Psychol.*, 2, 1908, 411ff.

<sup>13</sup> Titchener, *Philos. Stud.*, 20, 1902, 382ff.

<sup>14</sup> S. P. Hayes, *Amer. J. Psychol.*, 17, 1906, 358-393.

<sup>15</sup> Cohn, *op. cit.*

<sup>16</sup> Geissler, *op. cit.*

<sup>17</sup> M. Barrett, *Psychol. Rev.*, 21, 1914, 278-294.

<sup>18</sup> Cf. E. C. Sanford, The Vernier Chronoscope, *Amer. J. Psychol.*, 9, 1897-1898, 191-197.

connected with the exposure-apparatus in such a way that one of the pendulums was automatically released by an electromagnet when the shutter was raised. The other pendulum was similarly released when the reaction key was pressed by O.

O sat at a distance of 1.5 m. from the screen. The experiment took place in a dark-room. Light was furnished by two 75-watt frosted type C-2 Mazda daylight lamps, about 75 cm. behind O and a little to the right and left.

The actual procedure was as follows. At the beginning of each experiment, the ready signal was given by E who sat in the rear of the screen and manipulated the apparatus. About two seconds later, E raised the shutter and presented a pair of stimuli for three seconds. O, seated comfortably in his chair, faced the screen and announced his judgment by reacting upon a key with his right hand and stating subsequently whether his judgment has been "left" or "right." Immediately after recording O's judgment and reaction time, E shifted the stimuli and set the chronoscope for the next exposure. An interval from twenty to thirty seconds was necessary.

The experiment was continued in this fashion for the first thirty-five minutes of each session, after which a short intermission was given. The remaining time was chiefly devoted to taking introspections. B, D and F wrote their reports; M and P dictated them. It was possible to obtain about fifty comparisons, and one or more introspective reports in an hour.

*Instructions.*—The instructions finally adopted were type-written and read by O at the beginning of each session.

Instruction I. (For the comparison of colors.)

"In each experiment after the ready signal two colors will be exposed for a short time in the square openings of the screen. You are to give yourself up entirely to color-quality. You are to assume the attitude of passively living the color. You are not to be critical toward the stimulus or to concern yourself with extrinsic references, but to take the color simply in its own right. *Immediately* after the colors are perceived, indicate the one which you prefer as the more pleasant by pressing the key and by saying the word 'left' or the word 'right,' according as the left or the right color is preferred.

"Whenever you think you have failed to live up to this instruction, indicate that fact to the experimenter."

Instruction II. (For the comparison of forms.)

Same as Instruction I with "form" substituted for "color," and "form-aspect" substituted for "color-quality."

Instruction III. (For the comparison of color-forms.)

Same as Instruction I with "color-form" substituted for "color" and "color-quality."

When introspection was desired, the following instruction was given orally:

"In the next experiment, describe in detail your affective experience, with particular reference to the nature and mechanism of your judgment."

The observers were further instructed to indicate unambiguously cases of doubt, indecision, or of equality. D, F, M, and P were frequently unable to report upon the more pleasant stimulus, since each member of the pair was actually unpleasant; they were then allowed to base their preference upon the less unpleasant stimulus.

*Order of experiment.*—The experiment was divided into five parts. Parts I and V were planned to test the permanence of O's judgments upon colors and forms throughout the experimental period. The main experiment consisted of Parts II, III, and IV. Part II dealt with colors; Part III, with forms; and Part IV, with color-forms. The order for all O's but M is outlined below. M observed in the reversed order and did not participate in Parts I and V.

#### Part I.

Series S. The comparison of colors.

Instruction I. Stimuli, Group I (q. r.)

21 comparisons of colors in one spatial order, i. e., each of the 7 colors compared with each of the 6 other colors,  $7 \times 6/2 = 21$ ; and the repetition of the same in reversed spatial order, 42 comparisons in all.

Series N. The comparison of forms.

Instruction II. Stimuli, Group II (q. r.)

21 comparisons of forms in one spatial order, i. e., each of the 7 forms compared with each of the 6 other forms,  $7 \times 6/2 = 21$ ; and the repetition of the same in reversed spatial order, 42 comparisons in all.

Part II. The comparison of colors. Identical with Part I, Series S, except as to stimuli.

Instruction I. Stimuli, Group III (q. r.)

Series	Stimuli							
a	BB	GG	YY	OO	CC	RR	LL	21 comparisons in each of 2 spatial orders for each series; $2 \times 21 = 42$ comparisons in each series, $7 \times 6 = 42$ comparisons in all
e	BB	GG	YY	OO	CC	RR	LL	
c	BB	GG	YY	OO	CC	RR	LL	
o	BB	GG	YY	OO	CC	RR	LL	
r	BB	GG	YY	OO	CC	RR	LL	

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Part III. The comparison of forms. Identical with Part I, Series N, except as to stimuli.

Instruction II. Stimuli, Group III.

Series	Stimuli								
R	Ra	Rb	Rc	Rd	Re	Rf	Rg		21 comparisons in each of 2 spatial orders for each series; $2 \times 21 = 42$ comparisons in each series, $7 \times 42 = 294$ comparisons in all.
O	Oa	Ob	Oc	Od	Oe	Of	Og		
Y	Ya	Yb	Yc	Yd	Ye	Yf	Yg		
G	Ga	Gb	Gc	Gd	Ge	Gf	Gg		
C	Ca	Cb	Cc	Cd	Ce	Cf	Cg		
B	Ba	Bb	Bc	Bd	Be	Bf	Bg		
P	Pa	Pb	Pc	Pd	Pe	Pf	Pg		

Part IV. The comparison of color-forms.

Instruction III. Stimuli, Group III. Series 1 to 24 inclusive.

1176 comparisons of color forms in one spatial order only, i. e., each of the 49 color-forms compared with each of the other color-forms,  $49 \times 48/2 = 1176$ . Series 1 and 2 consisted of 42 comparisons each; series 3, of 63; and each of the rest, of 49.

Part V.

Series S' = Repetition of Part I, Series S.

Series N' = Repetition of Part I, Series N.

The stimuli were presented in haphazard order, with the precaution that no stimulus should appear twice in succession. The same stimuli, differently arranged, were used in Parts II, III, and IV. Thus, the isolation of color (Part II) and of form (Part III) was accomplished solely by the direction of attention. It would, of course, be ideal if we were able to present formless colors and colorless forms for the experiments on colors and forms. The use of the same stimuli in all parts of the main experiment did not, however, introduce any measurable disadvantage in the investigation, for the observers acquired skill in isolating under attention the color or form from the color-form and in attending to the one or the other separately.

### AFFECTIVE JUDGMENT UNDER THE METHOD OF PAIRED COMPARISONS

At the outset of this investigation, we had no definite knowledge regarding the practicability of the method of paired comparisons in dealing with our particular problem, nor did we have any exact conception of the nature of affections induced under this method. The choice of the method was quite arbitrary; it was, however, the only purely psychological method available, the only method that experimentalists might agree upon as furnishing the conditions or definition of the



generally conceded affective process. We found, as have our predecessors, that the results obtained with this method are of a positive character as far as their quantitative aspect is concerned, and that pleasantness and unpleasantness are not only reportable, but that judgments upon them may be nearly as immediate as judgments upon sensory impressions. Nevertheless the observers worked under very different attitudes and, so far as their introspective reports are admissible as evidence, we found their judgments based upon different sorts of psychological processes. This divergence in type of judgment led to less inconsistency in the nature of the data than might be expected, although the introduction of a critical introspective attitude undoubtedly accounts for the instability of some of the results. A characterization of these differences is in place here, since it reflects the limitations of the method; a critical study of the introspections which show these differences must, however, wait for a later article.

In a preliminary survey of the introspective data of our four practiced subjects, it appears that the affective judgments may be of two distinct types which depend upon two different attitudes,—a sensorial attitude and an objective attitude. The sensorial attitude presumably grew out of an instruction for introspection. Either the observers understood the instruction that they were sometimes to report introspectively upon the judging process to mean that they were to judge processes, or else the general introspective habits of the laboratory operated toward a similar self-instruction. The fact seems to be that the observers who sought to base their judgments upon introspectable material tended actually to base them upon sensory process, and that this introspective attitude is actually a sensorial attitude. Observer B's judgments were of this type. He approached the experiment with a firm conviction that if the affection is an existential mental process, it must be directly observable by introspection and his attitude throughout the experiment was characteristically introspective. He found, however, that pleasantness and unpleasantness were for him nonexistent in this psychological sense, that they were always 'meanings' or conscious attitudes based definitely under these particular conditions upon some particular sensory pattern. In order to make an affective judgment, however, it was always necessary for him to have some essential sensory cue, and his reaction-times, which were comparatively long, reflect this intervention of a sensory mediator between the perception of the stimulus and the arousal of the judgment. D also worked in the sensorial atti-

tude, characterizing affections as meanings carried by organic sensations. P reported them as observable psychological elements, but concomitant with organic and kinaesthetic processes. F, on the other hand, approximated the objective attitude, under which the stimulus is focal and introspected process is of secondary account. This attitude is in the spirit of the major instructions and F followed these instructions uncritically and was very receptive. His judgments after a little practice became mechanized and quick, as might have been expected, since there appeared to be no process-intermediary to retard the course. Although he became, during the course of the experiment, convinced that judgments of preference might be made without the presence of either of the affective qualities, he believed, nevertheless, that pleasantness and unpleasantness do exist as mental elements.

These results are especially suggestive in so far as they throw light upon the question of the psychological status of affection and the manner in which the problem of meaning enters into the problem of feeling. It is nothing new for observers in the same experiment to disagree over the nature of affection,<sup>19</sup> and the applicability of the introspective method is seriously open to question. Nevertheless the problem needs to be faced in the light of protocols obtained from observers trained in the introspective method and with their wits keen to note the incursions of meaning and *Kundgabe* upon the field of existential process. This issue remains; it is not immediately involved in the present problem, and it seems preferable to reserve it for separate treatment later.

It has seemed advisable tentatively to adopt the term "affective tendency" in this study because the entities involved (number of preferences) are statistical data and not immediately given and because the conceptual bases of the affective judgment must necessarily be as little directly observable as are the associative and determining tendencies. The difficulties and divergences of our observers emphasize again the elusiveness of the immediately observable affective datum, and the experiment as a whole brings to the fore what seems to be the available material at the bottom of the experimentalist's belief in affection, *viz.*, the statistical relative frequencies of preferences. And the term tendency, so it seems, is used by psychologists to represent those mental entities that appear thus in statistical settings.

<sup>19</sup> Nakashima, Contributions to the Study of the Affective Processes, *Amer. J. Psychol.*, 20, 1909, 180ff.

TABLE I

## PERMANENCY OF AFFECTIVE JUDGMENT

Percents of agreement between early series (Part I; S, N) and late series (Part V; S', N'). S and S' are for colored squares; N and N' for "neutral" forms. Obs.: B, D, F, P. Colors O and P omitted in ["B (revised)."]

	B	D	F	P	B (Revised)
S & S'	64.3	90.5	100.0	92.9	90.0
N & N'	90.5	95.2	100.0	88.1	90.5

*Degree of Permanency of Affective Judgment (Results of Parts I and V; Instructions I and II.)*

Table I indicates in terms of percentages of agreement,<sup>20</sup> the degree of permanence of O's affective judgments on colors and on forms just before (Part I) and after (Part V) the main experiment. Inspection will show clearly that, except in case of B, the influence of time,—as might be expected from the result of Bradford,<sup>21</sup>—is too small to change the preferential order of colors and forms. The low degree of consistency of B's judgments on colors is due apparently to a change in his preference for purple and orange during the progress of experiment. The omission of orange and purple from calculation of B's results gives a much higher percentage of agreement for colors without causing any change for forms (Table I, last column).

Of this change B remarked: "Purple became more pleasant than it used to be. I liked it better than red." Since the objective conditions of experiment have been kept fairly constant throughout and since no other salient cause is discoverable, we are justified in attributing B's change to the relative inconstancy of his attitude during the long interval of time. On the other hand, having shown that the degree of consistency of our observers' affective judgments in general is high and that therefore the preferential order for colors and for forms is not the result of transient conditions, we may

<sup>20</sup> See W. S. Foster and K. Riese, Tridimensional Theory of Feeling from the Standpoint of Typical Experiences, *Amer. J. Psychol.*, 27, 1915, 161, for the calculation of these. Our "per cent of agreement" is obtained by multiplying their "degree of similarity" by 100.

<sup>21</sup> E. J. C. Bradford, A Note on the Relation and Aesthetic Value of the Perceptive Types in Color Appreciation, *ibid.*, 24, 1913, 545-554.



TABLE IV

MUTUAL INDEPENDENCE OF COLOR AND FORM: OBS. F.

See Legend of Table II

Av. for color = 94.8; M. V. = 2.2							
COLOR							
		b	c	d	e	f	g Form
O	90.5	95.2	90.5	95.2	90.5	100.0	100.0 a
Y	95.2	95.2	95.2	95.2	95.2	95.2	95.2 b
G	90.5	90.5	95.2	95.2	100.0	90.5	90.5 c
C	95.2	85.7	90.5	85.7	95.2	95.2	95.2 d
B	95.2	95.2	100.0	95.2	90.5	90.5	90.5 e
P	95.2	95.2	100.0	95.2	90.5	100.0	100.0 f
Color	R	O	Y	G	C	B	

## FORM

Av. for form = 93.7; M. V. = 3.3

TABLE V

MUTUAL INDEPENDENCE OF COLOR AND FORM: OBS. M.

See Legend of Table II

Av. for color = 82.0; M. V. = 5.5							
COLOR							
		b	c	d	e	f	g Form
O	90.5	76.2	71.4	81.0	76.2	76.2	81.0 a
Y	71.5	76.2	85.7	85.7	81.0	88.1	90.5 b
G	76.2	76.2	85.7	85.7	76.2	76.2	76.2 c
C	88.1	76.2	90.5	76.2	85.7	90.5	90.5 d
B	76.2	85.7	71.4	85.7	76.2	76.2	76.2 e
P	81.0	88.1	76.2	81.0	76.2	90.5	81.0 f
Color	R	O	Y	G	C	B	

## FORM

Av. for form = 78.8; M. V. = 6.0

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TABLE VI

MUTUAL INDEPENDENCE OF COLOR AND FORM: OBS. P.

See Legend of Table II

Av. for color = 91.8; M. V. = 3.4							
COLOR							
	b	c	d	e	f	g	Form
O	85.7	97.6	81.0	90.5	92.9	97.6	92.9
Y	95.2	90.5	85.7	92.9	95.2	100.0	92.9
G	90.5	92.9	88.1	92.9	90.5	85.7	88.1
C	95.2	90.5	90.5	95.2	92.9	92.9	85.7
B	90.5	95.2	95.2	92.9	95.2	95.2	92.9
P	90.5	90.5	85.7	95.2	95.2	90.5	92.9
Color	R	O	Y	G	C	B	

FORM

Av. for form = 91.9; M. V. = 2.7

TABLE VII

MUTUAL INDEPENDENCE OF COLOR AND FORM: REVISED RESULT  
OF OBS. B.

See Legend of Table II. Colors O and P omitted because of inconstancy.

Av. for color = 86.7; M. V. = 5.6							
COLOR							
	b	c	d	e	f	g	Form
Y	90.5	95.0	85.0	90.0	80.0	95.0	95.0
G	88.1	92.9	85.0	90.0	85.0	90.0	100.0
C	88.1	92.9	100.0	85.0	70.0	85.0	85.0
B	81.0	90.5	95.2	95.2	75.0	90.0	90.0
						85.0	85.0
						90.0	90.0
Color	R	Y	G	C			

FORM

Av. for form = 91.4; M. V. = 3.8

*Mutual Independence of Color and Form in Conditioning  
Affective Tendency of Color and Form respectively  
under Attentive Isolation (Results of Parts  
II and III, Instructions I and II.)*

The results of Parts II and III are summarized in Tables II-VII. Each number in the upper right half of these tables is a percentage of agreement between the preferential order of colors of one form and the preferential order of the same colors with another form. The numbers in the lower left half are percentages of agreement of the preferential order of forms for pairs of colors. For example, in Table II, the figure 88.1 below 'f' and opposite 'a' is the percentage of agreement between the order of colors of series f (colors of form f) and the order of colors of series d (colors of form d); 90.5 above "R" and opposite "O" in the same table is the agreement between the preferential order of forms colored R and the order of forms colored O.

The percentages of agreement between the various series for both color and form are very high, ranging from 61.9 to 100.0 in case of the former, and from 66.7 to 100.0 in the latter. The averages of all the observers are 87.9 for color and 88.1 for form with the M. V's 3.9 and 3.7 respectively. These agreements are high enough to warrant a conclusion that when color-forms are presented for affective comparison of colors only, the forms have practically no influence upon the preferential order of these colors, provided the observer's attitude remains constant throughout the task, and that color similarly has practically no effect upon form. Color and form, then, may be said to be each independent of the other in conditioning affective tendency where each is isolated from the other by attentive abstraction.

This conclusion follows even in the face of relatively low degree of consistency of M's affective judgments on color and form and of B's on color. In B's case, as we have noted, purple and orange proved always the disturbing factors in his otherwise regular preferences. The M. V's of the scores of these colors were each greater than one-fourth of their averages. The inconsistency is plainly conditioned upon the nature of these two colors and not in any way upon form. (See Table VII, 'revised result' for B.) The case of M is of more significance, as these experiments were performed after she had completed 1176 comparisons of color-forms, and she was therefore expected to have a fixed set of preferential orders. However, there is no reason to assume that her failure to attain a higher degree of consistency is to be attributed to the mutual influence of color and form, exhibited



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by no other observer; rather it is reasonable to believe that, being untrained in psychological experimentation, she misunderstood the instructions, attempting to secure speed rather than accuracy. Moreover, it may be that she worked with various criteria of pleasantness, without sufficient discrimination, and thus gave inconstant results. These indications are borne out by her extraordinarily short reaction times and her frequent premature judgments, as well as by her own introspections, in which not infrequently are found such sentences as: "I chose the right in spite of myself," "the left was so impressive that I judged it as more pleasant, though I knew I liked the right better." It is also pertinent to observe that she had no decided preference among the forms as she did among the colors, and that the degree of consistency for forms is therefore even lower than for colors.

TABLE VIII  
AFFECTIVE VALUES OF COLOR-FORMS: OBS. B.

Number of preferences under method of paired comparisons for every colored form with attention on color-form (Instruction III). Rows show comparisons of forms for every color; columns show comparisons of colors for every form.

	a	b	c	d	e	f	g	Av.	M.V.
R	32.5	48.0	29.5	35.5	38.0	37.5	42.0	37.6	4.37
O	10.0	24.5	1.5	11.5	25.5	10.5	19.5	14.7	7.24
Y	17.0	41.0	17.0	25.5	27.0	15.5	29.5	24.6	6.98
G	24.5	36.0	25.0	28.0	34.5	28.5	36.5	30.4	4.50
C	2.5	25.0	3.5	7.0	11.5	3.0	10.0	8.9	5.68
B	14.5	39.0	18.5	24.5	30.0	17.5	34.0	25.4	7.64
P	20.5	38.0	13.0	25.0	31.0	21.0	35.5	26.3	7.33
Av.	17.3	35.8	15.4	22.4	28.2	19.1	29.6		6.25
M. V.	7.26	6.45	8.08	7.53	5.90	8.51	8.49	7.46	

TABLE IX  
AFFECTIVE VALUES OF COLOR-FORMS: OBS. D.  
See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	35.0	35.0	41.0	41.0	38.0	33.0	37.5	37.2	2.47
O	8.0	1.0	18.0	16.0	11.5	0.0	2.0	8.1	6.08
Y	15.5	7.0	30.0	28.0	22.0	4.0	7.0	16.2	8.96
G	12.0	8.0	28.0	25.0	18.0	6.5	11.0	15.5	7.00
C	25.0	21.0	34.0	33.5	27.0	16.0	23.0	25.6	5.02
B	45.0	42.0	48.0	46.0	45.0	42.0	43.0	44.5	1.79
P	25.0	15.5	32.0	31.0	20.0	7.0	16.0	20.9	7.20
Av.	23.6	18.5	33.0	31.5	25.9	15.5	19.9		5.50
M.V.	10.12	12.14	6.86	7.43	9.20	12.71	12.49	10.14	

TABLE X  
AFFECTIVE VALUES OF COLOR-FORMS: OBS. F.  
See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	38	37	47	35	31	19	43	35.7	6.33
O	32	27	44	27	22	14	42	30.0	8.29
Y	21	19	37	14	11	7	31	19.7	8.24
G	6	2	13	4	1	0	7	4.7	3.39
C	16	14	27	12	8	7	25	15.7	6.10
B	41	40	48	37	33	23	46	38.3	6.24
P	26	24	38	21	15	10	33	23.9	7.31
Av.	25.7	23.4	36.1	21.6	17.3	11.4	32.4		6.56
M.V.	9.76	9.94	9.37	11.37	9.76	6.20	9.80		9.46

TABLE XI  
AFFECTIVE VALUES OF COLOR-FORMS: OBS. M.  
See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	29.0	45.0	36.0	41.0	41.0	27.0	43.0	37.4	5.80
O	13.0	24.0	19.0	18.0	27.5	17.5	24.5	20.5	4.14
Y	10.0	35.0	28.0	24.0	26.0	17.0	32.0	24.6	6.49
G	10.0	12.0	6.5	3.5	11.0	5.0	20.0	9.7	4.04
C	14.5	25.0	14.5	13.0	11.5	7.5	15.0	14.4	3.22
B	34.0	47.0	44.0	40.0	42.0	32.0	44.0	40.4	4.37
P	12.5	30.0	26.0	24.0	22.5	10.5	21.0	20.9	5.39
Av.	17.6	31.1	24.9	23.4	25.9	16.6	28.5		4.78
M.V.	7.96	9.59	9.88	10.16	9.37	7.69	9.57		9.17

TABLE XII  
AFFECTIVE VALUES OF COLOR-FORMS: OBS. P.  
See Legend of Table VIII

	a	b	c	d	e	f	g	Av.	M.V.
R	27.5	34.0	35.0	35.0	45.0	16.5	47.0	34.3	7.10
O	24.0	28.0	29.0	36.0	39.0	17.0	44.0	31.0	7.43
Y	21.0	23.0	23.0	32.0	36.0	17.0	40.0	27.4	7.35
G	6.5	5.0	6.5	8.5	10.0	2.5	13.5	7.5	2.79
C	7.5	8.5	8.5	8.5	11.0	1.0	17.5	8.9	3.04
B	33.0	38.0	37.0	40.0	46.0	27.0	47.0	38.3	5.18
P	19.0	15.0	16.5	23.0	30.0	10.5	30.0	20.6	6.08
Av.	19.8	21.6	22.2	26.7	31.0	13.1	34.1		5.57
M.V.	7.53	10.41	10.04	10.89	12.00	7.20	11.94		9.99

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TABLE XIII

AFFECTIVE VALUES OF COLOR-FORMS: REVISED RESULT OF OBS. B  
See legend of Table VIII. Colors O and P omitted because of inconsistency.

	a	b	c	d	e	f	g	Av.	M.V.
R	22.5	34.0	19.0	23.5	26.0	24.5	29.0	25.5	3.21
Y	11.5	28.5	9.0	16.5	17.0	10.5	19.5	16.1	4.92
G	15.5	25.5	17.0	19.0	25.0	20.5	25.0	21.1	3.51
C	1.5	16.5	1.0	4.0	7.0	2.5	6.5	5.6	3.80
B	9.5	27.5	11.0	17.5	20.0	8.5	23.5	16.8	6.10
Av.	12.1	26.4	11.4	16.1	19.0	13.3	20.7		4.31
M.V.	5.52	4.32	5.48	4.84	5.60	7.36	6.16	5.61	

## *Concurrent Operation of Color and Form upon Affective Tendency (Parts II, III and IV).*

*Affective values of color-forms. (Part IV; Instruction III.)*  
—Tables VIII-XIII inclusive give the result of Part IV, the experiment upon the forty-nine stimuli with attention upon "color-form." In each table the figures in the first seven rows and columns are the absolute frequencies of choice made upon these color-forms by each observer. Further, these tables are so arranged that each row contains the scores of seven color-forms, different from one another in form and identical in color, and thus show implicitly the preferential order of seven forms, all of the same color; while, conversely, each column exhibits the preferential order of seven colors, all in the same form. The averages and M.V.'s are respectively placed in the rows and columns so named. For example, let us take Table VIII (Obs. B). The figures 32.5, 48, 29.5, *etc.*, respectively under a, b, c, *etc.*, in the row R indicate the scores of the color-forms, Ra, Rb, Rc, *etc.*, establishing the preferential order of the forms, a, b, c, *etc.*, for the color red. Likewise, the figures, 48, 24.5, 41, *etc.*, respectively opposite R, O, Y, *etc.*, in the column b may be taken either as the number of choices of the color-forms, Rb, Ob, Yb, *etc.*, or as indicative of the preferential order of the colors, R, O, Y, *etc.*, in the form b, the circle.

*Effect of instruction upon preference. (Comparison of Parts II and III with IV.)*—In order to measure the influence of the direction of attention upon affective tendency, the preferential orders of colors with attention directed upon color only (Part II, Instruction I) and of forms with atten-

TABLE XIV

## EFFECT OF DIRECTION OF ATTENTION

Correlation for colors between ranks with attention on color-quality (Instruction I) and ranks with attention on color-form (Instruction III). "Av." = av. correlation and av. P.E. "M. V." = M.V. of "Av." "Correl. Av." = correlation of av. of all forms for every obs. "B (revised)" = B's results with colors O and P omitted.

Obs.	B		D		F		M		P		Av. for All obs.		B (revised)	
	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.
a	0.86	0.07	0.92	0.04	1.00	0.00	0.43	0.22	0.96	0.02	0.83	0.07	0.88	0.0
b	0.56	0.17	0.88	0.06	1.00	0.00	0.87	0.07	0.99	0.01	0.86	0.06	0.55	0.2
c	0.90	0.05	0.99	0.01	0.96	0.02	0.93	0.04	0.98	0.01	0.95	0.02	0.90	0.0
d	0.46	0.21	0.94	0.03	0.99	0.01	0.90	0.05	0.94	0.03	0.85	0.07	0.88	0.0
e	0.64	0.16	0.94	0.03	0.96	0.02	0.74	0.12	1.00	0.00	0.86	0.07	0.90	0.0
f	0.63	0.16	0.85	0.08	0.99	0.01	0.81	0.09	0.83	0.08	0.82	0.08	0.90	0.0
g	0.67	0.15	1.00	0.00	1.00	0.00	0.88	0.06	0.93	0.04	0.90	0.05	0.95	0.0
Av.....	0.68	0.14	0.93	0.03	0.99	0.01	0.89	0.09	0.95	0.03	0.87	0.06	0.85	0.0
M. V.....	0.12		0.04		0.01		0.12		0.04		0.03		0.09	
Correl. Av....	0.74	0.12	1.00	0.00	1.00	0.00	0.96	0.02	1.00	0.00	0.94	0.03	0.90	0.0

TABLE XV

## EFFECT OF DIRECTION OF ATTENTION

Correlation for forms between ranks with attention on form-aspect (Instruction II) and ranks with attention on color-form (Instruction III). "Av." = av. correlation and av. P.E. "M. V." = M.V. of "Av." "Correl. Av." = correlation of av. of all colors for every obs. "B (revised)" = B's results with colors O and P omitted.

Obs.	B		D		F		M		P		Av. for All obs.		B (revised)	
	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.	$\rho$	P.E.
R	0.89	0.05	0.94	0.03	1.00	0.00	0.26	0.25	0.92	0.04	0.80	0.04	0.90	0.05
O	0.86	0.07	0.96	0.02	0.94	0.03	0.56	0.18	0.89	0.05	0.84	0.07	0.96	0.02
Y	0.88	0.06	0.93	0.04	0.99	0.01	0.39	0.23	0.96	0.02	0.83	0.07	0.96	0.02
G	0.89	0.05	0.96	0.02	0.89	0.05	0.21	0.26	0.88	0.06	0.77	0.09	0.92	0.04
C	0.86	0.07	0.96	0.02	1.00	0.00	0.19	0.26	0.96	0.02	0.79	0.07	0.96	0.02
B	0.82	0.09	0.98	0.01	0.99	0.01	0.62	0.17	0.99	0.01	0.88	0.06	0.75	0.12
P	0.96	0.02	0.96	0.02	0.99	0.01	0.77	0.11	0.80	0.10	0.90	0.05	0.90	0.05
Av.....	0.88	0.05	0.95	0.02	0.97	0.02	0.43	0.21	0.92	0.03	0.83	0.07	0.90	0.03
M. V.....	0.03		0.02		0.03		0.19		0.05		0.04		0.06	
Correl. Av....	1.00	0.00	1.00	0.00	1.00	0.00	0.46	0.21	0.96	0.02	0.89	0.05	1.00	0.00

tion directed upon form only (Part III, Instruction II) were compared by the method of rank-differences with the results for color and form respectively with attention upon 'color-form' (Part IV, Instruction III). The correlations are indicated in Tables XIV (color) and XV (form).<sup>22</sup> In table XIV, 0.90 under " $\rho$ " and opposite "c," for instance, is the

coefficient of correlation between the preferential orders of colors of the form c obtained in Part II and in Part IV; the figure 0.05 adjacent to it is the probable error of this  $\rho$ . Similarly, in Table XV, the figure 0.89 under " " and opposite "R" is the coefficient of correlation between the preferential orders of forms of the color red obtained in Part II and in Part IV, etc.

Examination of these tables reveals the fact that the coefficients of correlation, whether they be between the preferential orders of colors or of forms, are high for the observers D, F, and P, never falling below 0.80 and never with the P.E. exceeding one tenth of  $\rho$ . Indeed, for these observers, the correlation is perfect or nearly so when computed for the average ranks of colors or of forms. (Table XIV, row 'Correl. Av.', and Table XV, row 'Correl. Av.'). With B (color) and M (both color and form), the correlation is less significant than with the other observers.

It might be argued *a priori* that the poor result of M is due mainly to the fact that she had worked with the color-forms before she worked with the separate colors and forms, and that thus her judgments of the color-forms were relatively independent of the affective tendencies intrinsic to the colors and forms. Since Geissler, however, has already proved in his investigation of the influence of the affective tone of single colors on the affective tone of their combinations that there is no difference in the actual result whether the individual colors are presented before or after the color-pairs,<sup>23</sup> the writer is inclined to question the validity of any such claim. The more salient causes for the instability of her judgments seem to lie in the precarious nature of her attitude and her apparent lack of decided preference for any particular forms (see above). B's low correlation for the colors (0.77) results from the inconstancy of his judgments with the orange and purple stimuli. The recalculation of his data after eliminating the judgments on all the orange and purple stimuli raises the coefficient of correlation (Tables XIV and XV, last column) greatly between Part II and IV and between Part III and IV.

Taken all in all, there is a high degree of correspondence between the results of Parts II and III, and Part IV for all the observers, thus indicating that color and form operate as affective conditions independently of the direction of attention.

*Degree of independence of color and form in conditioning affective tendency of color-form.*—The failure to attain per-

<sup>22</sup> The formulae used were:  $\rho = 1 - \frac{6 \sum D^2}{n(n^2 - 1)}$ , and  
P.E.  $\rho = 0.706 \frac{1 - \rho^2}{n}$ .

<sup>23</sup> Geissler, *op. cit.*

fect correlations in the foregoing analysis may be attributed either to fluctuations of the observers' attitude under a given *Aufgabe*, or to the mutual effect of color and form upon the preferential order for each when the two are combined and judged as color-forms. Accordingly, if for the sake of simplicity we ignore, for a moment, the first of these factors (although there is every reason to believe that it was an important cause in effecting the inconstant results) and consider the data as indicative of mutual effects only, then the coefficients of correlation may be looked upon as the indices of the degrees of independence of color and form, one from another, as they condition the affective tendencies of the color-forms; and, conversely, the differences of these coefficients from unity may be taken as the measure of the degree of their mutual effect. It may be readily seen in Tables XIV and XV that, except in the cases of B and M, the effect is not very marked (ave. effects of forms on colors =  $1.00 - 0.87 = 0.13$ , of colors on forms =  $1.00 - 0.83 = 0.17$ ). The variations (M.V.'s) between the effects of different colors and forms are comparatively large; yet no definite relation can be found between the relative effects of these colors and forms, and the relative intensities of their own affective tendencies. It is doubtful whether these variations are at all the result of interaction of color and form. That they might easily be due to vacillations in the observer's attitude appears from the fact that they vary from observer to observer according to the order of the inconstancy of attitude. (See analysis of Parts I, II and III). At any rate we may conclude that as far as relative pleasantness is concerned, color and form are, in the main, mutually independent in conditioning affective tendency of color-form.

TABLE XVI

RELATIVE EFFECTIVENESS OF SEPARATE COLORS IN CONDITIONING AFFECTIVE TENDENCY OF COLOR-FORM

Inversely measured by M. V. of different forms for every color. Values taken from last columns, Tables VIII-XIII. Rank orders of effectiveness in parentheses.

Obs.:	B	D	F	M	P	Av. for All obs.	B (revised)
For the 7 forms in:							
R	4.37 (1)	2.47 (2)	6.33 (4)	5.80 (6)	7.10 (5)	5.21 (4)	3.21 (1)
O	7.24 (5)	6.08 (4)	8.29 (7)	4.14 (3)	7.43 (7)	6.64 (5)	...
Y	6.98 (4)	8.96 (7)	8.24 (6)	6.49 (7)	7.35 (6)	7.59 (7)	4.92 (4)
G	4.50 (2)	7.00 (5)	3.39 (1)	4.04 (2)	2.79 (1)	4.34 (1)	3.51 (2)
C	5.68 (3)	5.02 (3)	6.10 (2)	3.22 (1)	3.04 (2)	4.61 (2)	3.80 (3)
B	7.64 (7)	1.79 (1)	6.24 (3)	4.37 (4)	5.18 (3)	5.04 (3)	6.10 (5)
P	7.33 (6)	7.20 (6)	7.31 (5)	5.39 (5)	6.08 (4)	6.66 (6)	....
Av.	6.25	5.50	6.56	4.78	5.57	5.73	4.31



TABLE XVII

RELATIVE EFFECTIVENESS OF SEPARATE FORMS IN CONDITIONING AFFECTIVE TENDENCY OF COLOR-FORM

Inversely measured by M.V. of different colors for every form. Values taken from last rows, Tables VIII-XIII. Rank orders of effectiveness in parentheses.

Obs.:	B	D	F	M	P	Av. for All Obs.	B (revised)
For the 7 colors in:							
a	7.26 (3)	10.12 (4)	9.76 (4)	7.96 (2)	7.53 (2)	8.53 (2)	5.52 (4)
b	6.45 (2)	12.14 (5)	9.94 (6)	9.59 (5)	10.41 (4)	9.51 (6)	4.32 (1)
c	8.08 (5)	6.86 (1)	9.37 (2)	9.88 (6)	10.04 (3)	8.85 (3)	5.48 (3)
d	7.53 (4)	7.43 (2)	11.37 (7)	10.16 (7)	10.89 (5)	9.48 (5)	4.84 (2)
e	5.90 (1)	9.20 (3)	9.76 (3)	9.37 (3)	12.00 (7)	9.25 (4)	5.60 (5)
f	8.51 (7)	12.71 (7)	6.20 (1)	7.69 (1)	7.20 (1)	8.46 (1)	7.36 (7)
g	8.49 (6)	12.49 (6)	9.80 (5)	9.57 (4)	11.84 (6)	10.44 (7)	6.16 (6)
Av.	7.47	10.14	9.46	9.17	9.99	9.24	5.61

*Dominance of color or form as independently conditioning affective tendency of color-form.*—Let us inquire next whether color or form is more dominant in independently conditioning the affective tendency of color-form, and further, as to the relative effectiveness of separate colors and forms in such an operation.

It is evident that if the colors and forms were equally effective, the scatter of the scores of the forms in each color would tend to be equal to that of the colors in each form; and, if colors and forms were unequally effective, the scatter would be different. Again, if any color or form were more effective than the others, the scores of the forms of that color or of the colors of that form would cluster more closely together than the scores of the forms of any other colors or of the colors of any other forms. Obviously, therefore, the effectiveness of each of the seven colors is measured by the M.V. of the scores of the seven forms used in that color. A scale of the relative effectiveness of these colors may be based upon the comparison of these M.V.'s, with effectiveness inversely proportional to the M.V.'s. In the same way, the effectiveness of each of the seven forms can be measured in terms of the M.V. of the scores of the seven colors used in that form, and a scale similarly constructed for them.

These M.V.'s have been computed and are indicated in the right-hand columns and in the bottom rows of Tables VIII-XIII. They are reproduced in Tables XVI and XVII with the ranks (in parentheses) of the colors and forms according to their relative effectiveness. On the whole, the colors thus appear more effective than the forms in determining the affective tendencies of the color-forms.



The relative effectiveness of every color and form (Tables XVI and XVII) has been compared with its relative pleasantness (Part II, Instruction I, and Part III, Instruction II) with the following result:

The first three most effective colors are for the observer:

- B, the 2 most pleasant and the 1 least pleasant colors.
- D, the 2 most pleasant and the 1 moderately pleasant colors.
- F, the 1 most pleasant and the 2 least pleasant colors.
- M, the 1 most pleasant and the 2 least pleasant colors.
- P, the 1 most pleasant and the 2 least pleasant colors.

The first three most effective forms are for the observer:

- B, the forms which rank A, I, and 6.
- D, the 3 most pleasant forms.
- M, the 1 most pleasant and the 2 least pleasant forms.
- M, the 1 most pleasant and the 1 least pleasant and 1 moderately pleasant forms.
- P, the 3 least pleasant forms.

In other words the extremes, i.e. the most pleasant and the least pleasant colors and forms, were the most effective. These extremes must represent the most intensive affective degrees, since all observers except B testified that the least pleasant stimuli were actually intensively unpleasant. It appears, therefore, that the dominance of color or form as independently determining the affective tendency of color-form depends directly upon the intensity of its pleasantness and unpleasantness.

*Summation of affective tendencies.*—We have seen that the high degree of correspondence between the results of Part II and III (attention on color or form alone) and of Part IV (attention on color-form) indicates that both color and form were simultaneously effective in conditioning the pleasantness of a colored form when the attention was directed by instruction upon both the color and form aspects; and that color and form operate simultaneously in conditioning affective judgment in the same manner that each operates when attended to separately. We have seen further that the relative effectiveness of color and form in simultaneous operation is dependent upon the effectiveness of each when operating in attentive isolation. Now to say that color and form thus work independently and simultaneously to establish affective tendency in the way in which each works separately is to state that the two summate. Since we are dealing here with ranks and lack exact measurements of the amount of the affective tendencies involved, we can not, of course, state that summation occurs in any exact arithmetical sense, but the general law that their effects are algebraically additive can not be

denied. Remembering that four of the five observers made judgments at times upon the relative unpleasantness of the stimuli, we may lay down the rule: *Within the dimension of pleasantness and unpleasantness, the affective tendency of color-form varies approximately with the algebraical sum of the affective tendencies of its constituent color and form.*

TABLE XVIII

## EFFECT OF DIRECTION OF ATTENTION: PRELIMINARY EXPERIMENT

Preferential order of colors: correlation between ranks for colored squares, and ranks for colors of various forms. (12 obs.; 1st 4 women.)

Obs.:	1	2	3	4	5	6	7	8	9	10	11	12
$\rho$ :	0.97	0.94	0.93	0.97	0.93	0.97	0.89	0.90	0.94	0.99	0.99	0.97

TABLE XIX

## EFFECT OF DIRECTION OF ATTENTION: PRELIMINARY EXPERIMENT

Preferential order of forms: correlation between ranks for "neutral" forms, and ranks for forms of various colors. (12 obs.; 1st 4 women)

Obs.:	1	2	3	4	5	6	7	8	9	10	11	12
$\rho$ :	0.86	0.94	0.86	0.96	1.00	1.00	1.00	0.86	0.86	1.00	0.94	0.88

*Confirmation by an Experiment under Diffused Daylight.*—A comparable result was obtained in a preliminary experiment, which was conducted under uncontrolled diffused daylight but with laboratory conditions otherwise similar to those described above. There were 12 observers, 8 men and 4 women, all of whom except one had had only a little previous training in psychological observation. Color preferences were determined for these observers with a series of colored squares; form preferences were determined by the use of a series of forms outlined in black ink on a grey ground. The results were examined to see whether the judgments given for the 49 colored forms occurred as if dependent upon the concurrent independent summative operation of the tendencies effective in the first two series. As indicated in Tables XVIII and XIX, the co-efficients of correlation between the results on colors and forms and the results on color-forms are, without a single exception, very high for all the observers and support convincingly the conclusion reached in the foregoing analyses.

TABLE XX

AV. REACTION-TIMES (SECS.) FOR ALL COLORS OF EVERY FORM

Form	B	D	F	P	Av. 4 obs.	M
S	2.18	1.05	1.15	0.89	1.32	0.45
a	1.88	1.23	1.05	0.91	1.27	0.49
b	2.15	1.09	0.80	0.91	1.24	0.49
c	1.94	1.17	0.67	0.59	1.09	0.50
d	1.76	1.01	0.76	0.71	1.06	0.44
e	1.71	0.69	0.70	0.66	0.94	0.45
f	1.83	0.92	0.57	0.84	1.04	0.48
g	1.53	0.78	0.53	0.77	0.90	0.47
Av.	1.87	0.99	0.78	0.79	1.11	0.47
M.V.	0.17	0.15	0.16	0.11	0.15	0.02

TABLE XXI

AV. REACTION-TIMES (SECS.) FOR ALL FORMS OF EVERY COLOR

Color	B	D	F	P	Av. 4 obs.	M
N	2.19	1.05	0.68	1.25	1.29	0.64
R	2.08	0.92	0.73	0.78	1.13	0.60
O	1.19	0.80	0.63	1.05	0.92	0.65
Y	1.00	0.69	0.78	0.95	0.86	0.51
G	1.26	0.90	0.79	0.92	0.97	0.65
C	1.33	0.76	0.60	1.01	0.93	0.57
B	1.21	0.75	0.70	1.11	0.94	0.51
P	1.17	0.79	0.50	0.95	0.85	0.57
Av.	1.43	0.83	0.68	1.00	0.99	0.59
M.V.	0.35	0.10	0.09	0.10	0.16	0.04

## TIME-RELATIONS OF AFFECTIVE JUDGMENTS

*Speed.*—The data indicating the speed of affective judgments are summarized in Tables XX-XXII. They are fairly comparable with the results of Washburn<sup>24</sup> and Nakashima.<sup>25</sup> Miss Washburn and her collaborators found, with a group of thirty observers, that the average of the average reaction times

<sup>24</sup> H. Potter, R. Tuttle, and M. Washburn, The speed of affective judgments, *Amer. J. Psychol.*, 25, 1914, 288-290.

<sup>25</sup> Nakashima, Time relations of affective processes, *Psychol. Rev.*, 16, 1909, 303-339.

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for all the judgments of pleasantness on colors taken together was 1.4 secs. The individual averages ranged from 1 to 2.5. For unpleasantness the average was 1.4, ranging from 1 to 2.5. With another group of twenty-five observers, the average of the average reaction times for judgments of pleasantness, exclusive of extreme judgments, was 1.6; the longest individual average for pleasantness was 2.7 secs., and the shortest

TABLE XXII

AV. REACTION-TIMES (SECS.) FOR COLOR-FORMS FRACTIONATED TO SHOW PRACTICE-EFFECTS

Series	B	D	F	P	Av. 4 obs.	M
1	1.95	1.17	0.94	1.27	1.33	1.14
2	1.86	0.83	0.74	1.22	1.16	0.79
3	1.74	1.02	0.68	1.40	1.21	0.59
4	1.01	0.97	0.82	1.09	0.97	0.69
5	1.69	0.91	0.66	0.87	1.03	0.60
6	1.61	0.84	0.66	0.88	1.00	0.60
7	1.35	0.84	0.67	1.07	0.98	0.58
8	1.09	1.03	0.57	1.05	0.94	0.59
Av. 1-8	1.54	0.95	0.72	1.11	1.08	0.70
9	1.12	1.63	0.56	1.02	1.08	0.64
10	1.40	0.78	0.57	1.00	0.94	0.71
11	1.38	0.83	0.63	0.82	0.92	0.75
12	1.69	0.67	0.60	0.87	0.96	0.50
13	1.56	0.92	0.54	0.99	1.00	0.57
14	2.01	0.85	0.48	0.84	1.05	0.49
15	1.64	0.87	0.61	0.92	1.01	0.39
16	1.54	0.96	0.58	0.82	0.98	0.40
Av. 9-16	1.54	0.94	0.57	0.91	0.99	0.56
17	1.42	0.82	0.50	0.74	0.87	0.34
18	1.66	0.79	0.50	0.87	0.96	0.38
19	1.56	0.75	0.57	0.70	0.90	0.41
20	1.40	0.84	0.56	0.73	0.88	0.46
21	1.58	0.74	0.52	0.82	0.92	0.45
22	1.27	0.90	0.52	0.72	0.85	0.43
23	1.48	0.76	0.60	0.90	0.94	0.52
24	1.45	0.79	0.48	0.85	0.89	0.50
Av. 17-24	1.48	0.80	0.53	0.79	0.90	0.43
Av. of all	1.52	0.90	0.61	0.93	0.99	0.56
M.V.	0.20	0.12	0.08	0.14	0.14	0.13

0.9 secs. The average for unpleasantness, exclusive of extreme judgments, was 1.6 secs., the longest individual average for unpleasantness was 2.4 and the shortest 0.9. In both cases the speed varied with the intensity of affection. Nakashima by employing the direct reaction method found that the shortest time required for an affection to arise varied from 0.84 to 0.98 secs. for colors; and from 0.72 to 1.08 for geometrical figures. With our observers, the average of all the reaction times for colors is 1.11 secs., for forms, 0.99, and for color-forms, 0.99.<sup>26</sup> There are great individual differences due probably to the difference in attitude and in "affective sensitiveness" of the observers.

We find that the average of the reaction time for the comparisons of the most pleasant color-form with the 24 color-forms that come next in the order of preference ( $B=0.80$ ;  $D=0.72$ ;  $F=0.50$ ;  $P=0.61$  secs.) is greater than the average of the reaction time for the comparisons of the most pleasant color-form with the 24 least pleasant color-forms ( $B=0.74$ ;  $D=0.63$ ;  $F=0.45$ ;  $P=0.45$  secs.). It appears, therefore, that the speed of affective judgments under the method of paired comparisons varies with the degree of difference in the intensity or quality of the affective tendencies of the two stimuli compared. A similar relation would doubtless hold with sensory judgments.<sup>27</sup>

*Effect of Practice.*—The effect of practice is not striking except in the first two or three series. It is overshadowed by the presence of the daily fluctuations of reaction times, which are doubtless caused by the variations in psychophysical conditions determining the general efficiency of the observers. A practice-effect may, however, be made apparent by dividing the 24 series of experiments on color-forms into three groups according to the order in which they were conducted, and by comparing the averages of these reaction times. It will be seen (Table XXII) that these averages become shorter and shorter as they advance from the first to the last groups.

*Distribution of reaction times.*—The frequencies of reaction times in the experiments on colors, forms and color-forms are much skewed. Starting from the lower end, the distribution curve rises quickly to its maximum and falls slowly with

<sup>26</sup> M's results are excluded from the calculation of these averages, since with her the experiments were performed in the reversed temporal order.

<sup>27</sup> Cf. S. S. George, *Attitude in Relation to the Psychophysical Judgment*, *Amer. J. Psychol.* 28, 1917, p. 33.

the increase in the length of time. This skew is consistent with the fact that the reaction times are limited in negative deviation since they can scarcely be less than a considerable portion of a second, whereas there is no limitation to the delay that may occur in giving judgment.

#### CONCLUSIONS

I. The preferential orders of colors and forms are relatively permanent during a period extending over five months.

II. When color-forms are presented in some particular form for affective comparisons of colors only, the form has practically no influence upon the preferential orders of these colors.

III. When color-forms are presented in some particular color for affective comparisons of forms only, the color has practically no influence upon the preferential order of forms.

IV. As far as relative pleasantness is concerned, color and form are, in the main, mutually independent in conditioning affective tendency of color-form, even though simultaneously operative.

V. The dominance of color or form as independently determining the affective tendency of color-form probably depends directly upon the intensity of its pleasantness or unpleasantness.

VI. (Corollary of IV and V.) Within the dimension of pleasantness and unpleasantness, the affective tendency of color-form varies approximately with the algebraic sum of the affective tendencies of its constituent color and form.

VII. Two types of affective judgments, characterized by sensorial and objective attitudes, can be made under the method of paired comparisons.

VIII. The reaction times for affective judgments are more rapid in the immediate judgment of the objective attitude than in the mediate judgment of the sensorial attitude, and more rapid when the members compared differ widely in affective degree.

## BRENTANO AND WUNDT: EMPIRICAL AND EXPERIMENTAL PSYCHOLOGY<sup>1</sup>

By E. B. TITCHENER

§ 1. The year 1874 saw the publication of two books which, as the event has shown, were of first-rate importance for the development of modern psychology. Their authors, already in the full maturity of life, were men of settled reputation, fired as investigators with the zeal of research, endowed as teachers with a quite exceptional power to influence younger minds, ready as polemicists to cross swords with a Zeller or a Helmholtz. Yet one would look in vain for any sign of closer intellectual kinship between them; hardly, indeed, could one find a greater divergence either of tendency or of training. Psychology, seeing how much their work and example have done to assure her place among the sciences, may gladly confess her debt to both. The student of psychology, though his personal indebtedness be also twofold, must still make his choice for the one or the other. There is no middle way between Brentano and Wundt.<sup>2</sup>

Franz Brentano began his career as a catholic theologian. In 1867 he published an outline of the history of philosophy within the mediaeval church which sets forth, as clearly and sharply as the essay of thirty years later, his famous doc-

<sup>1</sup> The following paragraphs form the introduction to the first volume of my long-projected and long-delayed work upon Systematic Psychology. When I wrote them, Brentano and Wundt were still living. Brentano died at Zurich, March 17, 1917; Wundt died at Leipzig, Aug. 31, 1920.

<sup>2</sup> F. Brentano, *Psychologie vom empirischen Standpunkte* (henceforth cited as *PES*), i, 1874. Cf. the Biographical Note in F. Brentano, *The Origin of the Knowledge of Right and Wrong*, trs. C. Hague, 1902, 119 ff.; M. Heinze, *F. Brentanos Grundriss der Geschichte der Philosophie*, iv, 1906, 332 ff. W. Wundt, *Grundzüge der physiologischen Psychologie* (henceforth cited as *PP*), 1874. The first ten chapters of Wundt's work were issued in 1873 and are utilised by Brentano. For a bibliography of Wundt's scientific writings see *Amer. Journ. Psychol.* xix (1908) ff.; cf. Heinze, *op. cit.*, 322 ff.



trine of the four phases.<sup>3</sup> Early and late, however, his intellectual interest has centered in the philosophy of Aristotle. He came to psychology by way of an intensive study of the *De Anima*, and he has made the Aristotelian method his pattern of scientific procedure. We possess, unfortunately, only the first volume of his *Psychologie*: Brentano seems always to have preferred the spoken to the written word: but this volume, like everything else that he has given to the press, is complete in itself, the finished expression of his mature thought.

Wilhelm Wundt started out as a physiologist, interested in the special phenomena of nerve and muscle. In 1862 he had sought to lay the foundations of an 'experimental psychology' (the phrase now appears in print for the first time)<sup>4</sup> in a theory of sense-perception. Here he fell into the mistake to which every student of natural science is liable who turns, without due preparation, to the things of mind: the mistake, namely, of supposing that psychology is nothing more than an applied logic; and the mistake was repeated in a popular work upon human and animal psychology which followed on the heels of the technical volume. By 1874 he had definitely discarded this earlier view for the conception of psychology as an independent science. He still maintained, however, that the path to it leads through the anatomy and physiology of the nervous system.

Such, in briefest outline, were the conditions under which the two psychologies acquired their form and substance. We see, on the one hand, a man who has devoted his 'hours of solitary reflection' to ancient and mediaeval philosophy; we see, on the other hand, a man who has wrought out in the laboratory his contributions to the latest-born of the experimental sciences. They are both professors of philosophy, and they are both to range widely, in the future, over the varied fields of philosophical enquiry. Yet it would be wrong to suppose that the psychology to which they have now attained, and which, by a happy chance, they give to the world in the same year, represents merely an incident, even if it were the central incident, of their philosophical history. Psychology, on the contrary, has laid strong hands upon them, and is to dominate all their further thinking. Wundt, a gen-

<sup>3</sup> J. A. Möhler, *Kirchengeschichte*, ii, 1867, 539 f.; F. Brentano, *Die vier Phasen der Philosophie und ihr augenblicklicher Stand*, 1895. The four phases, repeated in the three great philosophical periods, are those of scientific construction, failure or perversion of the scientific interest, scepticism and mysticism.

<sup>4</sup> W. Wundt, *Beiträge zur Theorie der Sinneswahrnehmung*, 1862, vi.

eration later, will round off the manifold list of his books with the encyclopaedic folk-psychology, and Brentano never gives up the hope of a descriptive—to be followed, perhaps, at long last by a genetic—psychology as the ripe fruit of his studious old age.

§ 2. We shall better understand the nature of this choice which lies before us if we first note the points of resemblance between the two systems. For even in 1874 psychology was not in such bad case that Brentano and Wundt are always at variance. They agree that psychology holds a place of high importance in the fellowship of the sciences, and that it is logically prior to natural science.<sup>5</sup> They agree that it may dispense with the concept of substance and confine itself to an account of phenomena.<sup>6</sup> They reject the unconscious as a principle of psychological explanation.<sup>7</sup> They define the unity of consciousness in substantially the same terms.<sup>8</sup> So far there is agreement: and though the agreement is largely of a formal kind, and though a good deal of it has a negative ground in the reaction against Herbart, it serves nevertheless to mark out a common universe of discourse.

On the material side there is also agreement, with such difference of emphasis as the difference of authorship would lead us to expect. We find, for instance, that Brentano deals at length with the general method of psychology, and is at pains to distinguish inner perception from inner observation, while Wundt takes inner observation for granted and describes in detail only those special procedures which raise it to the rank of experiment.<sup>9</sup> We find that Wundt devotes much space to Fechnerian psychophysics, and interprets the psychophysical law as a general psychological law of relativity, while Brentano makes only incidental and critical mention of Fechner's work.<sup>10</sup> The differences are striking enough, but behind them lies agreement regarding the subject-matter of psychology. Even in the extreme case, where the one book emphasises what the other omits, difference does not of necessity mean disagreement. We find, again, that Wundt says nothing of a question which for Brentano is the essential problem of psychology as it was the first problem of psychophysics, the question of 'immortality,' of the continuance of our mental life after death, and conversely that Brentano fails

<sup>5</sup> *PES*, 24 ff.; 119; *PP*, 4, 863.

<sup>6</sup> *PES*, 10 ff.; *PP*, 9, 12, 20.

<sup>7</sup> *PES*, 133 ff.; *PP*, 644 f., 664, 708 f., 712, 790 ff.

<sup>8</sup> *PES*, 204 ff.; *PP*, 715 ff., 860 ff.

<sup>9</sup> *PES*, 34 ff., 184; *PP*, 1 ff.

<sup>10</sup> *PP*, 421; *PES*, 9 f., 87 ff.

to discuss Wundt's cardinal problem of attention. Yet Wundt had touched upon the question of immortality in his earlier writing, and Brentano plainly recognises that there is a problem of attention, although (as we may suppose) he has put off its discussion to his second volume.<sup>11</sup>

So the student of psychology who read these two books in their year of issue might, if he had made due allowance for the training and natural tendencies of the authors, have entertained a reasonable hope for the future of his science; and we ourselves, who see their differences far more plainly than was possible for him, may still hope that the main issue can be taken on common ground and fought out at close quarters.

§ 3. Brentano entitles his book 'psychology from the empirical standpoint,' and Wundt writes 'physiological psychology' on his title-page and suggests 'experimental psychology' in his text.<sup>12</sup> The adjectives do not greatly help us. For all experimental psychology is in the broad sense empirical, and a psychology which is in the narrow sense empirical may still have recourse to experiment. To show the real difference between the books, the difference that runs through their whole texture and composition, we need at this stage terms that are both familiar and clear; the time has not yet come for technicalities and definitions. We may say, as a first approximation, that Brentano's psychology is essentially a matter of argument, and that Wundt's is essentially a matter of description.

At the end of his discussion of method Brentano refers with approval to Aristotle's use of *aporiae*, of difficulties and objections, wherein a subject is viewed from various sides, and opinion is weighed against opinion and argument against argument, until by comparison of pros and cons a reasonable conclusion is reached.<sup>13</sup> This is, in the large, his own way of working. He appeals but rarely, and then only in general terms, to facts of observation. His rule is to find out what other psychologists have said, to submit their statements to a close logical scrutiny, and so by a process of sifting to pre-

<sup>11</sup> *PES*, 17 ff., 32 f., 95 f.; Wundt takes up the question of immortality (indirectly, it is true) in *Vorlesungen*, etc., ii, 1863, 436, 442; cf. the direct treatment in the later edition, 1892, 476 ff. Brentano recognises the problem of attention in *PES*, 91, 155; cf. 263, and C. Stumpf, *Tonpsychologie*, i, 1883, 68; ii, 1890, 279 f.

<sup>12</sup> *PP*, 3.

<sup>13</sup> *PES*, 96 f.; cf. J. S. Mill, Grote's Aristotle, *Fortnightly Rev.*, N. S. xiii, 1873, 48 ff. Brentano had earlier noted, with the same approval, the use of *aporiae* by Thomas Aquinas: see J. A. Möhler, *Kirchengeschichte*, ii, 1867, 555.

pare the reader's mind for a positive determination. When the ground has thus been cleared Brentano's doctrine, novel though it may be, has the appearance (so to say) of a necessary truth; we feel that we have duly considered the possibilities in the case and have come to the one rational decision; and if for conscience' sake we go on to deduce and to verify, we still are assured beforehand that everything will fit together within the system. Minor points may need to be expanded; even, perhaps, in the light of further *aporiae*, to be corrected; but the whole exposition gives the impression of finality.<sup>14</sup> It is no wonder, then, that many students have judged the author successful in his aim of writing, not Brentano's psychology, nor yet a national psychology, but—psychology.<sup>15</sup>

Wundt's book, on the contrary, abounds in facts of observation: anatomical facts, physiological facts, results of psychophysical and psychological experiment. Its introductory chapter is brief to the point of perfunctoriness, and criticism of psychological theories is packed away into fine-print paragraphs that, to all intents and purposes, are a series of appendices. There is, to be sure, a great deal of argument. Where the facts are scanty, they must not only be generously interpreted but must also be eked out by hypothesis; if a leading physiologist has mistaken the problem of sense-perception,

<sup>14</sup> I know of only three corrections that Brentano has made to his psychology. (1) In *PES* 292 degree of conviction, as intensity of judgment, is declared analogous to degree of intensity of love and hate (cf. 203); in the notes to *The Origin of the Knowledge of Right and Wrong* (1889), 1902, 52 f., this analogy is denied. (2) In *PES* 202 f. feeling is said to be always present along with ideation; the belief to the contrary is due to the mistaken preference of memory over inner perception (44); but in *Untersuchungen zur Sinnespsychologie*, 1907, 119, 124, the acts of the two higher senses are not intrinsically emotive. (3) In *PES* 115 the object upon which a psychical phenomenon is directed is not to be understood as *eine Realität*; but the notes appended to the reprinted section *Von der Klassifikation der psychischen Phänomene* (1911, 149) lay it down that "nie etwas anderes als Dinge, welche sämtlich unter denselben Begriff des Realen fallen, für psychische Beziehungen ein Objekt abgibt."—There would, no doubt, if the book were rewritten, be many other modifications of detail, and yet others if the second volume were undertaken; the discussion of the modi of ideation in the *Klassifikation* shows that Brentano had not in 1874 thought out the doctrine of his *Rk.* iii. In the main, nevertheless, the doctrine of 1874 has stood the test of Brentano's own continued reflection and of the attacks of critics.

Such an achievement is worthy of all admiration. Only we must add—those of us who challenge Brentano's premises—that even isolated changes are disconcerting. The first statement is so serenely confident, and the changes are again so confidently made!

<sup>15</sup> *PES*, vi.



he must be argued into a better way of thinking; in any case, the new science of experimental psychology must offer a bold front to her elder sisters.<sup>16</sup> The argument, none the less, is always secondary and oftentimes plainly tentative; so that the book as a whole gives the impression of incompleteness, of a first essay which can be improved when more work (and a great many suggestions of further work are thrown out<sup>17</sup>) has been accomplished. Hence it is no accident, but rather a direct reflex of the spirit in which the authors approached their task, that Brentano's volume still bears the date 1874 while Wundt's book, grown to nearly triple its original size, has come to a sixth edition.<sup>18</sup>

This thorough-going difference of argument and description means, of course, a radical difference of attitude toward psychology itself. It means that Brentano and Wundt, in spite of formal and material agreement, psychologise in different ways. Our next step, therefore, is to place ourselves inside the systems and to realise, so far as we may without too much detail, what manner of discipline they intend psychology to be. We have to choose: and the illustrations that follow will show the alternatives of choice in concrete and tangible form.

§ 4. Brentano defines psychology as the science of psychical phenomena. The term may easily be misleading: for the phenomena in question are very far from being static appearances. Generically they are activities; in the individual case they are acts. Hence they can properly be named only by an active verb. They fall into three fundamental classes: those, namely, of Ideating (I see, I hear, I imagine), of Judging (I acknowledge, I reject, I perceive, I recall), and of Loving-Hating (I feel, I wish, I resolve, I intend, I desire). We may use substantives if we will, and may speak of sensation and idea, memory and imagination, opinion, doubt, judgment, joy and sorrow, desire and aversion, intention and resolution; but we must always bear in mind that the psychical phenomenon is active, is a sensing or a doubting or a recalling or a willing.

It is true that we never have act without content. When we ideate, we sense or imagine something; when we judge,

<sup>16</sup> *PP*, Vorwort.

<sup>17</sup> *PP*, 284, 293, 314, 317, 373, 394, 399, etc., etc.

<sup>18</sup> See the prefaces to the successive editions of the *PP*. Even the sixth edition, as I have shown elsewhere (*Psych. Rev.*, xxiv, 1917, 52 f.), has not attained to systematic completion, and only in the fifth (*PP*, i, 1902, ix) did Wundt set himself definitely to the task of system-making.

we perceive something, acknowledge the truth of something, recall something; when we love or hate, we take interest in something, desire or repudiate something. This, however, is precisely the difference between psychical and physical phenomena. The latter are blank and inert: the color or figure or landscape that I see, the chord that I hear, the warmth or cold or odor that I sense, the like objects that I imagine, all these things are described when their given appearance is described; their appearance sums them up and exhausts them; they have no reference, and do not carry us beyond themselves. Psychical phenomena, on the other hand, are precisely characterised by relation to a content, by reference to an object; they contain an object intentionally within them; and this character of immanent objectivity, in virtue of which they are active, marks them off uniquely from the physical phenomena upon which they are directed or toward which they point. Even in cases where the content of a psychical phenomenon is not physical, but is another psychical phenomenon, the distinction holds good. For the act which becomes content or object of another act is not thereby deprived of its essential character; it is still active in its own right; and it is therefore by no means confusable with bare physical appearance.<sup>19</sup>

These are Brentano's views of the subject-matter of psychology. He begins by considering the alleged differences between physical and psychical, finds an adequate *differentia* of the psychical, and is therefore able to define psychology in terms of the matter with which it deals. He then reviews the principal classifications hitherto made of psychical phenomena, and arrives at a classification of his own, in which judgment is accorded independent rank, and feeling and will are bracketed under a single heading. Throughout the discussion his chief reliance is upon argument. To be sure, he takes the testimony of inner perception; but inner perception is not observation; it is rather a self-evident cognition or judgment; and as such it is, if we may use the phrase, of the same stuff as argument.<sup>20</sup> Psychological observation is possible for Brentano only when past acts are recalled in memory; then indeed, as he admits, even a sort of experimentation becomes possible. Not only, however, is memory subject to gross illusion, but the act of memory, once more, falls under the category of judgment, so that experiment itself takes place in the world

<sup>19</sup> PES, 23 f, 35, 101 ff, 161, 167, 256 ff. On the problem of natural science as an explanatory discipline, see 127 ff.

<sup>20</sup> PES, 35 ff, 181 ff (summary 202 f), 262. Cf. *Klassifikation*, 1911, 129.

of argument.<sup>21</sup> The empirical psychology thus employs the same psychical activities to establish the nature of its subject-matter and to discuss the variety of psychological opinion.

§ 5. For Wundt, psychology is a part of the science of life. Vital processes may be viewed from the outside, and then we have the subject-matter of physiology, or they may be viewed from within, and then we have the subject-matter of psychology.<sup>22</sup> The data, the items of this subject-matter, are always complex, and the task of experimental psychology is to analyse them into "the elementary psychical processes." If we know the elements, and can compare them with the resulting complexes, we may hope to understand the nature of integration, which according to Wundt is the distinguishing character of consciousness.<sup>23</sup>

Analysis of the processes of the inner life brings us, in the last resort, to pure sensations, constituted originally of intensity and quality. Sensations carry no reference; they look neither before nor after; they tell us nothing of their stimuli, whether external or organic, and nothing of their point of excitation, whether peripheral or central, nor do they forecast the ideas in which we find them synthetised. They simply run their course, qualitatively and intensively, and may be observed and described as they proceed.<sup>24</sup> Ideas, in their turn, are originally constituted of these sensations; there is nothing within or upon them to show whether they are ideas of imagination or perceptions.<sup>25</sup> Individual ideas differ psychologically from general ideas solely in the nature of their sensory constituents: in the former the complex of sensations is constant, in the latter it is variable.<sup>26</sup> Concepts are not "psychical formations" at all; if we psychologise them, we discover only their substitutes in consciousness, spoken or written words, accompanied by a vague and indeterminate feeling.<sup>27</sup> Judgments, in the same way, belong to logic, and not primarily to psychology; logic and psychology approximate only as a result of the parallel growth, long continued, of conceptual thinking and its expression in language; our "conscious psychological

<sup>21</sup> *PES*, 42 ff., 162, 169, 262; *Klassifikation*, 130.

<sup>22</sup> *PP*, 1 ff.

<sup>23</sup> *PP*, 5, 20, 717.

<sup>24</sup> *PP*, 273 ff., 484 f. When sensations enter into connection with one another, the third attribute of affective tone or sensory feeling is added. Intensity and quality are, however, the "more original" constituents.

<sup>25</sup> *PP*, 464 f.

<sup>26</sup> *PP*, 468.

<sup>27</sup> *PP*, 672.



processes" consist originally of nothing more than ideas and their connections.<sup>28</sup>

The trend of all this analysis is clear: Wundt is trying to describe mind, to show the stuff of which it is made, to reduce it to its lowest terms. When, however, he turns from analysis to synthesis, the exposition is less easy to follow. Sensations are integrated into ideas by a "psychical synthesis" which Wundt himself compares to a chemical synthesis and which critics have assimilated to Mill's "mental chemistry."<sup>29</sup> Ideas gain their objective reference by a "secondary act" which seems to consist, psychologically, in the simple addition of further ideas;<sup>30</sup> yet the objective reference is itself put, later on, to psychological purposes. Concepts and forms of intuition are made 'postulates' of advancing thought,<sup>31</sup> as if the logical and practical aspects of mind were necessarily implied in its given or phenomenal aspect, and as if the psychologist might shift from one aspect to another without breach of scientific continuity. But though we may puzzle over details, there is nothing obscure in the general situation. Wundt, like many others of his generation, is dazzled by the vast promise of the evolutionary principle;<sup>32</sup> 'original' is for him more or less what 'nascent' is for Spencer; the later must derive from the earlier, because that is the way of things, and the later has no other basis. Let us remember, all the same, that Wundt's primary effort is to describe, and that he falls back upon 'genetic explanation' only when some phase of the traditional subject-matter of psychology proves to be indescribable.

That, then, is one of the threads of Wundt's system. Even a descriptive psychology cannot, however, be written simply in terms of sensations and their modes and levels of psychological integration. For the field of consciousness, Wundt reminds us, is not uniformly illuminated: it shows a small bright area at its centre and a darker region round about; the ideas which occupy it differ in their conscious status. So arises the problem of attention. Descriptively—Wundt takes up the task of description piecemeal, in different contexts, as

<sup>28</sup> *PP.* 709 ff.

<sup>29</sup> *PP.* 484 f.; J. S. Mill, *A System of Logic*, 1843, bk. vi, ch. iv (ii, 1856, 429); *An Examination of Sir William Hamilton's Philosophy*, 1865, 286 f.; note in J. Mill, *Analysis of the Phenomena of the Human Mind*, i, 1869, 106 ff. The original source is D. Hartley, *Observations on Man*, 1749, pt. i, ch. i, sect. 2, prop. 12, cor. 1 (i, 1810, 77 f.).

<sup>30</sup> *PP.* 465.

<sup>31</sup> *PP.* 672, 680.

<sup>32</sup> *PP.* vi.

if it were 'on his conscience'—attention reduces to clearness of ideas and characteristic feelings of effort or strain.<sup>33</sup> It has two concrete manifestations, apperception and voluntary action; we speak of apperception when we are considering the internal course of ideas, and of voluntary action when we are considering the issue of an emotion in external movement.<sup>34</sup> Both forms of the attentional process are subject to conditions, and both are strictly correlated with physiological processes in the cerebral cortex; they therefore fall within the limits of a scientific psychology.<sup>35</sup> Yet psychologists have neglected them, and have paid the penalty of this neglect in inadequate psychology and untenable philosophy.<sup>36</sup>

We need not here trace the doctrine of attention further; we need not either debate whether the problem of attention is included in Wundt's formal statement of the task of experimental psychology. We may, however, as an illustration of the interweaving of the two systematic threads, glance at his treatment of the association of ideas. He begins, as we might expect, with mode of integration; and under this heading declares that the recognised laws, of similarity and of frequency of connection in space and time, are imperfect even as empirical generalisations. We find, it is true, two forms of association, distinguishable in the free play of fancy and in reflective thought. But the one is wider than association by similarity, in that the effective resemblance may reside in any and every sensory constituent of the ideas concerned, and especially in their affective tone, while the other reveals itself simply as an affair of habit. Wundt therefore proposes to term them, respectively, 'association by relationship' and 'association by habituation.' The new names, he maintains, are not indifferent; for they do fuller justice than the old to the facts of self-observation, and they also point us to the conditions of association in the central nervous substance.<sup>37</sup>

Here then is an improvement on the side of analysis and synthesis; but that is not enough. For ideas do not associate automatically, as it were of their own motion; the laws of association are, on the contrary, under the universal dominance of attention. And now there opens up, for experimental attack, a whole series of special problems which an empirical psychology, following only the single line of enquiry,

<sup>33</sup> *PP*, 717 ff., esp. 724.

<sup>34</sup> *PP*, 831, 835.

<sup>35</sup> *PP*, 720 f., 723 f., 834 f.

<sup>36</sup> *PP*, 792 f., 831 ff.

<sup>37</sup> *PP*, 788 ff.

must naturally miss. In their light we pass beyond associationism to a more faithful transcript of the 'course and connection of ideas';<sup>38</sup> and in like manner we avoid, in our psychology of will, the philosophical *impasse* of indeterminism.<sup>39</sup>—

These paragraphs express, in rough summary, the teaching of the Wundt of 1874. He does not give psychology a distinct and peculiar subject-matter; the difference between physiology and psychology lies simply in our point of view. Wundt had already published a comprehensive work upon physiology, and now that he has turned to psychology he carries his knowledge and method with him; he is convinced that the processes of the inner life are best set forth in close connection with those of the outer life, and that the results of inner observation are surest when the appliances of external observation, the procedures of physiology, are pressed into psychological service. He spends little time upon preliminaries, but gets as quickly as may be to the exposition of facts. Where facts are few or lacking, he seeks to supplement or to supply them by observations of his own. His primary aim in all cases is to describe the phenomena of mind as the physiologist describes the phenomena of the living body, to write down what is there, going on observably before him: witness his treatment of idea, of concept, of attention, of association. There is still great space for argument, and the argument, we must admit, is often influenced by previous habits of thought, by psychological tradition, by a certain tendency to round things off to a logical completeness, by a somewhat naïve trust in the principle of evolution. The argument, however, does not impress the reader as anything but secondary: Wundt is at once too dogmatic and too ready to change his views. The recurring need of further facts and the patchwork character of the argument suggest, both alike, that psychology, under his guidance, has still a long systematic road to travel.

§ 6. We have now viewed our two psychologies from within. Brentano, we have found, looks back over the past, weeds out its errors with a sympathetic hand, accepts from it whatever will stand the test of his criticism, and organises old truth and new into a system meant, in all essentials, to last as long as psychology shall be studied; Wundt, after he has acknowledged his debt to the past, turns away from it and plunges into the multifarious and detailed work of the laboratories, producing a psychology that is as much encyclo-

<sup>38</sup> *PP*, 793; cf. the earlier sections of ch. xix.

<sup>39</sup> *PP*, 837 f.

paedia as system, and that bears on its face the need for continual revision. Which of the two books holds the key to a science of psychology?

Brentano has all the advantage that comes with historical continuity. His doctrine of immanent objectivity goes back to Aristotle and the Schoolmen, and the classification of psychical acts into ideas, judgments, and phenomena of love and hate goes back to Descartes.<sup>40</sup> More than this: he can claim kinship with every psychologist, of whatever school, who has approached his subject from the technically 'empirical' standpoint. For the 'empirical' psychologist means to take mind as he finds it; and like the rest of the world, who are not psychologists, he finds it in use; he finds it actively at work in man's intercourse with nature and with his fellow-man, as well as in his discourse with himself. Terms may change and classifications may vary, but the items of classification are always activities, and the terms employed—faculties, capacities, powers, operations, functions, acts, states—all belong to the same logical universe. Brentano, innovator though he is, takes his place as of right in a great psychological community.<sup>41</sup>

To offset this advantage, and to justify his own break with tradition, Wundt holds out the promise of an experimental

<sup>40</sup> PES, 115 f.; *The Origin of the Knowledge of Right and Wrong*, 47.

<sup>41</sup> In spite of the remarks in §3 and in §6 below it may seem unjust to Brentano if, even in this preliminary sketch of the psychological issue, his interest in experiment is left without record. We note, then, that as early as 1874 he urged the establishment at Vienna of a psychological laboratory (*Ueber die Zukunft der Philosophie*, 1893, 47 f.); that he has published *Untersuchungen zur Sinnespsychologie* (1907) and in particular that he brought the Müller-Lyer illusion to the attention of psychologists (*Zeits. f. Psych. u. Phys. d. Sinnesorgane*, iii, 1892, 349); and that Stumpf, who was his pupil (*Ueberweg-Heinze*, iv, 1906, 334 f.), has given us the experimental *Tonpsychologie*. All this, however, does not prevent his being, in the narrow sense, an 'empirical' psychologist. Stumpf tells us that his own work is to "describe the psychical functions that are set in action by tones" (*Tonpsych.*, i, 1883, v) and declares later that "there cannot be a psychology of tones; only a psychology of tonal perceptions, tonal judgments, tonal feelings" (*Zur Einteilung der Wissenschaften*, 1907, 30). Brentano, even with a laboratory, would not have been, in Wundt's sense, an 'experimental' psychologist. We know, besides, something of Brentano's systematic programme. The empirical psychology is not to be concluded; it is to be supplemented and replaced by a 'descriptive' psychology (*The Origin*, etc., vii, 51 f.), fragments of which have appeared in *The Origin of the Knowledge of Right and Wrong* (dealing with the phenomena of love and hate and, in the Notes, with judgment) and in the *Untersuchungen* (sense-perception). This in turn is to be followed by an explanatory or 'genetic' psychology, a sample of which is given in *Das Genie*, 1892 (see *The Origin*, etc., 123).



method. He should have been more explicit: for technology as well as science—medicine as well as physiology, engineering as well as physics—makes use of experiment. His actual purpose, as we trace it in the chapters of his book, is to transform psychology into an experimental science of the strict type, a science that shall run parallel with experimental physiology.<sup>42</sup> He failed, no doubt, to see all that this purpose implied, and his earlier readers may be excused if they looked upon his work as an empirical psychology prefaced by anatomy and physiology and interspersed with psychophysical experiments. There is plenty of empirical psychology in the volume. If, however, we go behind the letter to the informing spirit; if we search out the common motive in Wundt's treatment of the familiar topics; if we carry ourselves back in thought to the scientific atmosphere of the seventies, and try in that atmosphere to formulate the purpose that stands out sharp and clear to our modern vision; then the real significance of the *Physiological Psychology* cannot be mistaken. It speaks the language of science, in the rigorous sense of the word, and it promises us in this sense a science of psychology.

But Brentano also speaks of a 'science' of psychology. Which of the two authors is in the right?

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<sup>42</sup> The substitution of folk-psychology for experiment in the study of the more complicated mental processes appears in the fourth edition (*PP*, i, 1893, 5); the reservation in regard to psychophysical parallelism in the fifth edition (*PP*, iii, 1903, 775 ff.).

## A PRELIMINARY STUDY OF THE RANGE OF VISUAL APPREHENSION

By SAMUEL W. FERNBERGER, University of Pennsylvania

Writers of psychological textbooks usually state that from four to six objects may be apprehended in a single grasp of attention.<sup>1</sup> It is not often that the range of attention is supposed to be greater than six, although a recent text states: "Under ordinary conditions from 6 to 8 objects are clearly distinguished simultaneously. The number may be increased with practice to about 15."<sup>2</sup> The emphasis upon the number six is derived from the pre-experimental work of Sir William Hamilton and his predecessors, but the later experiments seem not to have altered it.<sup>3</sup>

When one examines these measures of the so-called range of attention, however, one finds that the concepts are neither clear nor exact. The usual textbook statement would lead one to believe that all groups up to, say, 4, 5 or 6 objects are invariably capable of being apprehended correctly and that there is no way of assuring correct apprehension of a greater number. Nothing could be further from the case. Take Jevons' early results.<sup>4</sup> He threw at random a few beans

<sup>1</sup> E. B. Titchener, *A Beginner's Psychology*, 1915, 103; J. R. Angell, *Psychology*, 1904, 81; C. S. Myers, *A Text-book of Experimental Psychology*, 1911, 321; W. B. Pillsbury, *The Fundamentals of Psychology*, 1916, 247; R. M. Yerkes, *Introduction to Psychology*, 1911, 297.

<sup>2</sup> H. C. Warren, *Human Psychology*, 1919, 251.

<sup>3</sup> Sir W. Hamilton, *Lectures on Logic and Metaphysics*, 1859-60, I, 253 f. For a survey of the experimental work cf. J. McK. Cattell, *The Inertia of the Eye and Brain*, *Brain*, VIII, 1886, 248-404; B. Erdmann & R. Dodge, *Psychologische Untersuchungen über das Lesen*, 1898; G. M. Whipple, The Effect of Practice Upon the Range of Visual Attention and of Visual Apprehension, *Jour. of Ed. Psychol.*, I, 1910, 249-262; F. N. Freeman, Untersuchungen über den Aufmerksamkeitsumfang und die Zahlauffassung bei Kindern und Erwachsenen, *Päd. Psychol. Arbeit.*, 1910, 88-168; J. Zeitler, Tachistoskopis. Versuche über das Lesen, *Philos. Stud.*, XVI, 1900, 380-463.

<sup>4</sup> W. S. Jevons, The Power of Numerical Discrimination, *Nature*, III, 1871, 281 f. It will be noted that Jevons' results are very incompletely quoted by W. James, *The Principles of Psychology*, 1890, I, 406. James quotes only the results for the first four stimuli, namely, 3, 4, 5, and 6, while Jevons used stimuli up to 15 beans. The results as quoted by James are misleading.

into a box and sought at a glance to estimate their number. His results, thrown into the form of relative frequencies, are as follows:

Number of Beans	Per Cent. Correct Judgment
3	100
4	100
5	96
6	82
7	73
8	56
9	62
10	43
11	38
12	42
13	23
14	28
15	18

Jevons uses as the measure of the discrimination 100 *per cent* correct judgments and concludes that his limit is between 4 and 5 beans.

Cattell's experiments in the Wundtian laboratory yield the following results.

Number of Objects Exposed	PER CENT. CORRECT JUDGMENT			
	Lines	Digits	Letters	Short Words
2				60
3		90	87	23
4	90	74	63	
5	75	60	36	
6	75	34	23	

But here one cannot accurately use 100 *per cent* correct judgments as the measure of apprehension, because the experiments do not include those small values of the stimulus that would give the highest frequencies. One might extrapolate the function to 100 *per cent* if the frequencies indicated the exact nature of the curve.

In view of these facts, it appears that the statistical limen or threshold, used in the work on sensation and perception, is a more reliable and more readily determined measure of the so-called range of attention. The threshold is defined as that value of stimulus the sensing of which has a probability of 0.5; or that value of stimulus which is as likely to be sensed as not. The problem of the range of apprehension presents, statistically, the same problem as does the terminal limen for sensation. In the case of sensation, one is interested in determining, let us say, the value in wave-frequency of the tone



which is sensed just half of the time and which is so high that it is just not sensed half of the time. Similarly, in the range of apprehension, one is interested in determining the number of objects, the correct apprehension of which has a probability of 0.5. The use of the psychophysical procedure in this case not only yields a measure of the range of apprehension that has a maximum degree of precision, but also ordinarily provides a measure of the degree of precision. It is curious that investigators, working outside the field of sensation, have not oftener used the tools of psychophysics that have been so highly developed for sensation. Williams<sup>5</sup> made such an application when he calculated an associative limen by means of the method of constant stimuli, but the extension is unusual.

Jevons' results, although they involve three inversions, can nevertheless, for purposes of rough comparison, be treated by the method of constant stimuli. For his data the limen is 10.28 beans and the coefficient of precision is 0.143,—a statement that is much more definite than Jevons' conclusion: "The limit of complete accuracy, if there were one, would be neither 4 nor 5, but half-way between them; but this is a result as puzzling as one of the uninterpretable symbols in mathematics, just, for instance, like the factorial of a factorial number. But I give it for what it may be worth."<sup>6</sup>

It must be remembered that this threshold will ordinarily be expressed in fractions of an object, even though it is not possible to prepare a fractional number of objects, of which the correct apprehension has a probability of 0.5. The limen is a *statistical limen*, a calculated quantity which summarizes observed results for purposes of scientific comparison. When one finds the upper threshold of tone to be, say, 18,264 *vs.*, one does not seek to construct a stimulus for this frequency in order to see whether it will give 50 *per cent* positive judgments. One is satisfied with the computed limen as part of a statistical account of observations already made.

The present paper presents some preliminary determinations of a limen of visual apprehension.<sup>7</sup> The stimuli were circular dots, 5 mm. in diameter. Four to twelve dots were arranged on cardboards in a haphazard order. Each group fell entirely within the range of foveal vision. These cards were exposed

<sup>5</sup> H. D. Williams, On the Calculation of an Associative Limen, *Amer. Jour. of Psychol.*, XXIX, 1918, 219-226.

<sup>6</sup> Jevons, *op. cit.*

<sup>7</sup> These experiments were performed in the Laboratory of Experimental Psychology at Clark University during the spring of 1920.

by means of a Whipple tachistoscope.<sup>8</sup> A pre-exposure fixation point was used. The subject was seated at a distance of 2.5 m. from the apparatus.

Three series of stimuli were employed. They were:

Series I. Black dots on white background with an exposure time of 100 sigma.

Series II. Black dots on a medium-grey background with an exposure time of 100 sigma.

Series III. Black dots on a white background with an exposure time of 60 sigma.

By comparing Series I and III we hoped to study the effects of the speed of exposure. By comparing the results of Series I and II we hoped to study the effect of the brightness of the stimulus.

Five subjects were employed. They were L. D. Boring, Ph. D. (Ob. B), C. C. Pratt, A. M. (Ob. P), S. Yokoyama, A. M. (Ob. Y), M. Bates, A. M. (Ob. Ba) and J. H. Alston, A. M. (Ob. A). The instructions were: "You will be shown successively, for a very short exposure, cards which will contain a varying number of black dots on a white (or grey) background. Immediately after the exposure you will report verbally the number of dots which you have apprehended. Be sure of your judgment and do not guess. The report 'I do not know' is admitted."

The judgments were recorded either as 'correct' or 'not correct.' Seven cards were shown in each series, four to ten dots to Obs. P, Ba and A; six to twelve dots to Obs. B and Y. Nine series of cards were prepared (63 cards in all); each series was shown an equal number of times; and the cards of each series were shuffled after the series had been presented. In Series I (white background, 100 sigma exposure) 250 judgments for each number of dots were obtained from each subject. In Series II (grey background, 100 sigma exposure) 150 judgments were obtained, and in Series III (white background, 60 sigma exposure) 150 judgments were obtained from each subject. The results were fractionated into groups of 50 judgments on each stimulus and each fraction was treated separately by Urban's procedure for the method of constant stimuli.<sup>9</sup>

<sup>8</sup> For a description of this apparatus cf. G. M. Whipple, *Manual of Mental and Physical Tests*, I, 1914, 264 f.

<sup>9</sup> For a description of this form of mathematical treatment cf. F. M. Urban, *Hilfstabellen für die Konstanzmethode*, *Arch. f. d. ges. Psychol.*, XXIV, 1912, 236-243.

TABLE I

		OB. B.						
		Number of Dots						
	Fractions	6	7	8	9	10	11	12
Series I	I	0.96	0.88	0.72	0.58	0.44		
	II	1.00	0.96	0.88	0.52	0.46	0.28	0.12
	III	1.00	0.96	0.84	0.64	0.46	0.28	0.20
	IV	1.00	0.96	0.74	0.58	0.44	0.22	0.22
	V	1.00	0.98	0.84	0.56	0.36	0.18	0.10
	Ave.	0.99	0.95	0.80	0.58	0.43	0.24	0.16
Series II	VI	1.00	0.98	0.86	0.56	0.34	0.22	0.08
	VII	1.00	0.98	0.90	0.60	0.38	0.20	0.10
	VIII	1.00	0.98	0.90	0.74	0.54	0.30	0.12
	Ave.	1.00	0.98	0.89	0.63	0.42	0.24	0.10
Series III	IX	1.00	0.98	0.92	0.74	0.46	0.32	0.16
	X	1.00	1.00	0.92	0.86	0.56	0.32	0.22
	XI	1.00	0.98	0.94	0.74	0.52	0.28	0.18
	Ave.	1.00	0.99	0.93	0.78	0.51	0.31	0.19

TABLE II

		OB. P.						
		Number of Dots						
	Fractions	4	5	6	7	8	9	10
Series I	I	1.00	0.92	0.84	0.60	0.06	0.00	0.00
	II	1.00	0.98	0.88	0.60	0.04	0.00	0.00
	III	1.00	0.98	0.92	0.48	0.08	0.00	0.00
	IV	0.96	0.88	0.82	0.56	0.08	0.00	0.02
	V	0.96	0.94	0.90	0.64	0.20	0.00	0.00
	Ave.	0.98	0.94	0.87	0.58	0.09	0.00	0.00
Series II	VI	1.00	0.98	0.86	0.62	0.30	0.00	0.00
	VII	0.98	0.96	0.80	0.56	0.06	0.02	0.00
	VIII	0.98	0.94	0.78	0.48	0.16	0.02	0.00
	Ave.	0.99	0.96	0.81	0.55	0.17	0.01	0.00
Series III	IX	0.98	0.96	0.80	0.58	0.24	0.04	0.00
	X	1.00	0.96	0.88	0.68	0.30	0.06	0.00
	XI	1.00	0.96	0.92	0.62	0.28	0.04	0.00
	Ave.	0.99	0.96	0.87	0.63	0.27	0.05	0.00

TABLE III

OB. Y.								
Fractions		Number of Dots						
		6	7	8	9	10	11	12
Series I	I	0.98	0.94	0.86	0.72	0.54		
	II	0.98	0.94	0.90	0.68	0.64	0.42	0.20
	III	1.00	0.98	0.92	0.72	0.56	0.44	0.16
	IV	1.00	0.98	0.92	0.80	0.70	0.50	0.30
	V	1.00	1.00	0.98	0.90	0.72	0.56	0.30
Ave.		0.99	0.97	0.92	0.76	0.63	0.48	0.24
Series II	VI	1.00	0.98	0.96	0.88	0.74	0.56	0.34
	VII	1.00	0.98	0.94	0.92	0.82	0.54	0.42
	VIII	1.00	0.98	0.94	0.86	0.68	0.48	0.28
	Ave.	1.00	0.98	0.95	0.89	0.75	0.53	0.35
Series III	IX	1.00	0.98	0.92	0.88	0.72	0.48	0.24
	X	1.00	0.98	0.92	0.88	0.78	0.54	0.28
	XI	1.00	0.98	0.96	0.88	0.74	0.56	0.34
	Ave.	1.00	0.98	0.93	0.88	0.75	0.53	0.29

TABLE IV

OB. BA.								
Fractions		Number of Dots						
		4	5	6	7	8	9	10
Series I	I	0.98	0.96	0.90	0.74	0.14	0.00	0.00
	II	1.00	0.96	0.96	0.64	0.12	0.04	0.00
	III	1.00	0.98	0.88	0.68	0.24	0.02	0.00
	IV	1.00	0.96	0.92	0.70	0.22	0.02	0.00
	V	1.00	0.96	0.90	0.72	0.40	0.16	0.02
Ave.		1.00	0.96	0.91	0.70	0.22	0.05	0.00
Series II	VI	1.00	0.98	0.94	0.76	0.48	0.22	0.02
	VII	1.00	0.98	0.92	0.76	0.46	0.22	0.06
	VIII	1.00	0.98	0.94	0.72	0.48	0.24	0.06
	Ave.	1.00	0.98	0.93	0.75	0.47	0.23	0.05
Series III	IX	1.00	0.96	0.88	0.76	0.42	0.24	0.06
	X	1.00	0.98	0.92	0.74	0.24	0.12	0.04
	XI	1.00	0.98	0.94	0.78	0.38	0.18	0.06
	Ave.	1.00	0.97	0.91	0.76	0.35	0.18	0.05

TABLE V

OB. A.								
Fractions		Number of Dots						
		4	5	6	7	8	9	10
Series I	I	0.68	0.52	0.38	0.22	0.20	0.16	0.10
	II	0.88	0.72	0.50	0.36	0.30	0.16	0.10
	III	0.96	0.88	0.72	0.36	0.34	0.24	0.22
	IV	0.96	0.94	0.68	0.56	0.44	0.24	0.22
	V	1.00	0.94	0.82	0.60	0.42	0.24	0.20
	Ave.	0.90	0.80	0.62	0.42	0.34	0.21	0.17
Series II	VI	1.00	0.98	0.84	0.56	0.36	0.26	0.22
	VII	0.96	0.90	0.78	0.62	0.44	0.28	0.22
	VIII	0.98	0.94	0.80	0.64	0.38	0.20	0.16
	Ave.	0.98	0.94	0.81	0.61	0.39	0.25	0.20
Series III	IX	0.98	0.92	0.78	0.64	0.42	0.26	0.18
	X	1.00	0.98	0.92	0.76	0.56	0.28	0.20
	XI	1.00	0.96	0.92	0.72	0.48	0.28	0.18
	Ave.	0.99	0.95	0.87	0.71	0.49	0.27	0.19

The observed relative frequencies of correct judgments are to be found in Tables I-V, which give for each observer and for every fraction of 50 judgments the relative frequency of correct judgments on every number of dots employed. The averages for each series are averages of the fractions of that series. An examination of these tables shows that we are here dealing with a continuous function. In the case of every group for every observer, the relative frequencies of correct judgments are high for the low values of stimulus and decrease regularly as the number of dots increases. There is not a single inversion in the entire five tables.

These relations become more obvious when the results are thrown into the form of curves. Figure I gives these curves for Ob. Ba. The average relative frequencies (Table IV) of the three series are plotted. The curves resemble the *phi-gamma function*, but are asymmetrical in that the dispersion of the supraliminal portion is less than that of the subliminal part.

The numerical values of the thresholds (S) and of the coefficients of precision (h) are given in Tables VI-X. It will be observed that both the values of the observed relative

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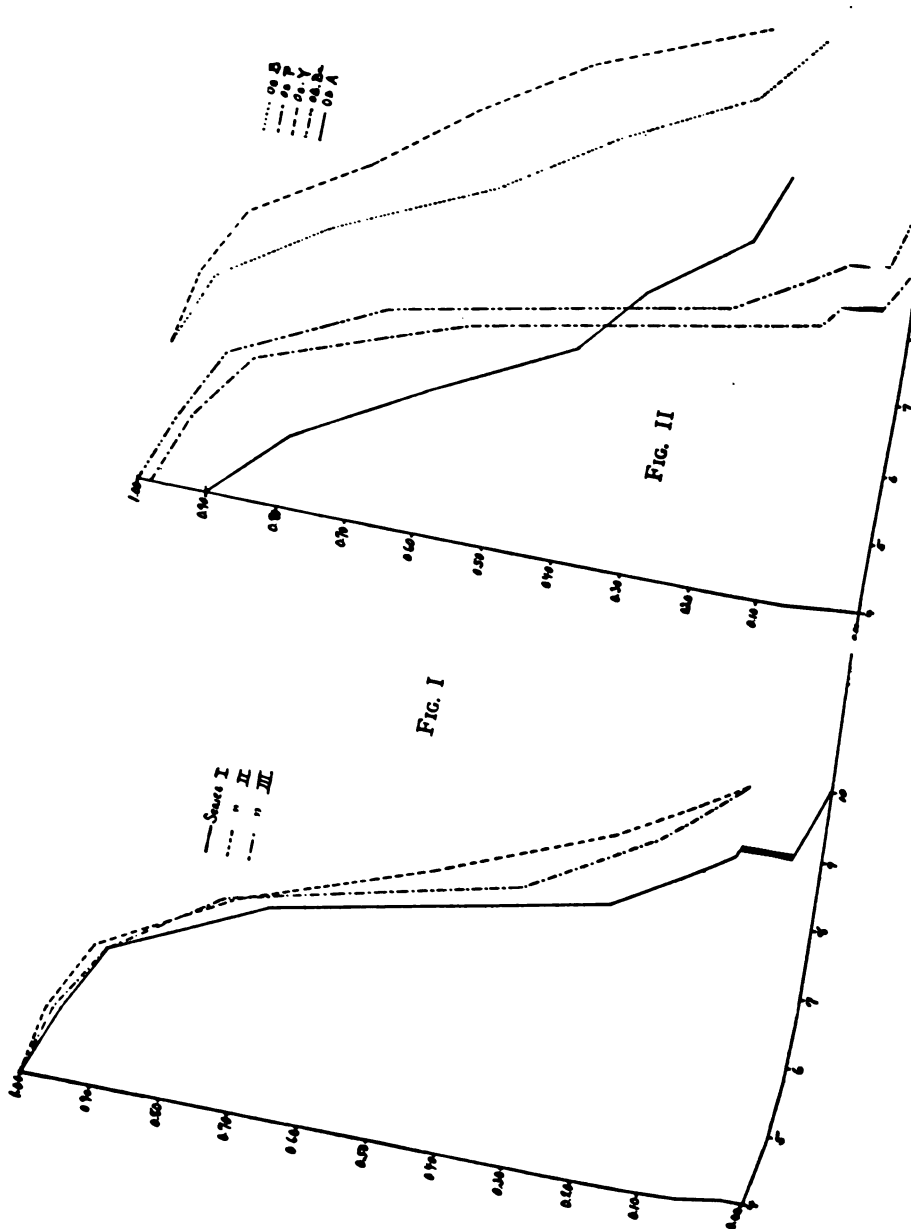


TABLE VI

OB. B.						
Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.314	9.499	0.441	9.554	0.421	10.140
II	0.385	9.755	0.454	9.683	0.416	10.442
III	0.349	9.969	0.434	10.122	0.429	10.204
IV	0.312	9.695				
V	0.450	9.551				
Ave.	0.362	9.694	0.443	9.786	0.422	10.262

TABLE VII

OB. P.						
Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.642	6.939	0.586	7.359	0.555	7.097
II	0.862	6.996	0.644	6.864	0.595	7.376
III	0.880	6.942	0.590	6.848	0.640	7.305
IV	0.501	6.883				
V	0.492	7.246				
Ave.	0.675	7.001	0.607	7.024	0.597	7.259

requencies and those of the coefficients of precision and the thresholds are relatively similar for the different series for each observer. It was expected that the form of the curves might vary for the different series, thus showing the influence of the brightness of the backgrounds and of the length of the exposure times. No such variation is present for any subject.

Probably the effect of practice obscures any slight differences that might otherwise occur between the series. Practice is evident in the higher values of both the threshold and the coefficient of precision. The effect of practice upon the



TABLE VIII

Ob. Y.						
Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.345	10.204	0.365	11.252	0.390	10.884
II	0.331	10.334	0.342	11.535	0.365	11.126
III	0.389	10.374	0.381	10.912	0.365	11.252
IV	0.341	10.954				
V	0.425	11.138				
Ave.	0.366	10.601	0.362	11.233	0.373	11.087

TABLE IX

Ob. BA.						
Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.581	7.282	0.556	7.906	0.469	7.830
II	0.700	7.146	0.518	7.909	0.592	7.565
III	0.700	7.289	0.509	7.934	0.553	7.835
IV	0.687	7.294				
V	0.532	7.650				
Ave.	0.640	7.332	0.527	7.916	0.538	7.743

coefficient of precision is noticeable when the average values of every subject, except Ob. P, are compared. The effect on the thresholds is marked for all subjects, especially for Ob. A. These results accord with former experiments on the effects of practice upon visual apprehension.<sup>10</sup>

<sup>10</sup> G. M. Whipple, The Effect of Practice Upon the Range of Visual Attention and of Visual Apprehension, *Jour. of Ed. Psychol.*, I, 1910, 249-262; K. M. Dallenbach, The Effect of Practice Upon Visual Apprehension in School Children, *Jour. of Ed. Psychol.*, V, 1914, 321-334, 387-404; K. M. Dallenbach, The Effect of Practice Upon Visual Apprehension in the Feeble-Minded, *Jour. of Ed. Psychol.*, X, 1919, 61-82; cf. *Journ. Exp. Psych.*, iii, 1920, 228 f.

TABLE X

OB. A.						
Fractions	Series I		Series II		Series III	
	h	S	h	S	h	S
I	0.204	4.974	0.336	7.757	0.338	7.768
II	0.276	6.435	0.303	7.825	0.413	8.276
III	0.290	7.291	0.373	7.660	0.400	8.102
IV	0.299	7.632				
V	0.339	7.776				
Ave.	0.282	6.822	0.337	7.747	0.384	8.049

Figure II gives a comparison of the results for the different observers. The psychometric functions for each subject in Series I are plotted together. The graphs illustrate the relative positions of the thresholds for the five observers. Ob. Y has the highest values, ranging for all of the three series about 11 dots. Next lower are the values for Ob. B, ranging about 10 dots. The values for Ob. Ba are about 7 to 8 dots; those for Ob. A in the neighborhood of 7 to 8 dots; and those for Ob. P are the lowest, being very close to 7 dots.

It seemed also of interest to determine just what were the mental processes underlying the formation of these judgments of visual apprehension. For this purpose, some introspections were obtained from each of the subjects. An analysis of these introspections reveals the following facts.

For Ob. B, the process was essentially one of utilizing the immediate memory after-image after the exposure was completed and of grouping the dots for purposes of apprehension. This grouping was present even for the lower values of stimulus (6 and 7 dots for this subject). The following introspection is typical for this observer:

"This time the judgment came very quickly and was accompanied by an attitude of great subjective certainty. The spot-pattern was very distinct and the dots were very uniformly black and very sharply outlined. After the exposure, there was a visual memory after-image left for a moment after the light had streaked by. The counting in vocal-motor terms began while this was still present. The left part of this image was most distinct and was counted first. Fixation, with accompanying kinaesthesia of effort, of the exposure square persisted until the counting was finished. The counting was in groups with eye-movements from the location of one group to that of the next as I counted."

The process for Ob. P were very rapid and the judgment was reported as having been formed during the time of the actual exposure. Eye-movement did not seem to be present but, even though the exposures were as short as 100 and 60 sigma, there was grouping. This grouping was present, apparently, in shifts of clearness and of focality. The following is a typical introspection:

"When the shutter was removed, there was intensive, but indistinct visual perception of a light and rather extended area. This first immediate visual perception was rapidly followed by a standing-out, with respect to clearness, of the central portion of the area. In this central portion, I perceived focally the three dots (which formed the upper portion of the figure). Then there was a rapid shift in focality in which I perceived very clearly the remaining dots of the figure. There was also vocal-motor imagery, very rapid, telescoped and non-focal, present with the changing focality of the dots, of 'four—seven.' This was then vocalized and general relaxation followed."

Ob. Y's processes were very similar to those of Ob. P. The processes were rapid and the judgment was reported as being completed by the end of the exposure-period. Grouping was present, again in terms of shifts of clearness and focality. The following introspection will illustrate the processes employed:

"I had a clear auditory perception of the 'ready' signal, which was followed immediately by the clear perception of the dots arranged in the form of an uneven square. The attention then shifted to the upper right-hand part of the figure. Rapid perception of four dots which stood out very clearly. Then I had a clear perception of three dots in the lower left-hand part of the figure. Then immediately I had a very clear perception of another group of three dots in the central part of the figure. Then followed the verbalization 'ten' which was followed by intensive pleasantness."

Observers Ba and A were not so highly trained in introspection as were the other subjects. Still their protocols give an insight into the processes present. Ob. Ba employed a process very similar to that reported by Ob. B. A visual memory after-image of the stimulus was present immediately after the exposure. Grouping was present in terms of clearness-shifts in this image. The following introspection illustrates the process employed:

"There was first a perception of the stimulus as a whole, no part of it being any clearer than any other part. This was followed by a visual memory after-image which lasted only a very short time. Then a true visual image of the stimulus came in. First the left-hand side was clearest and I had a vocal-motor 'four.' Then the right-hand side became clearer and I had a vocal-motor 'three.' Then the entire image became fairly clear and this was immediately followed by the verbalization of 'seven.'"

Observer A employed an immediate visual after-image, and grouping was present in terms of clearness-shifts in this image. The following typical introspection is given:

"Immediately after the exposure I had a visual memory after-image of the stimulus. At first three dots in the upper portion of the figure were very clear. This was followed almost simultaneously by a standing-out clearly of two dots in the lower portion of the figure. Then an image in which all five dots were clear and focal came in rather suddenly and gradually faded out. The judgment was then verbalized."

A consideration of these introspections reveals that grouping was always present for all of the observers. The exposure-times which were employed were too brief to admit eye-movements. Still the splitting up of the total stimulus into groups was accomplished by means of shifts of clearness and of focality. The observers may be divided into two distinct types. For Obs. Y and P this grouping of the dots by means of clearness shifts was reported as being accomplished during the actual exposure of the stimulus, and the judgment was completed by the end of the very brief exposure-time. For Obs. B, Ba and A, this grouping took place in a visual memory after-image which appeared immediately after the exposure was completed. One might hope here to discover a basis for the differences in the size of the limen between the different subjects but, unfortunately, the hope is disappointed. The grouping on the side of process is: Immediate type of judgment, Obs. Y and P; use of visual memory after-image, Obs. B, Ba and A. The grouping of subjects on the basis of limen-values is: Higher values, Obs. B and Y; lower values, Obs. Ba, P and A. The groups in the two cases do not agree. Ob. Y, who employed the immediate type of judgment, had the highest limen-values; while Ob. P, who employed the same type of process, had the lowest.

From a consideration of the introspections we are convinced that the range of attention is an erroneous title for this sort of experiment. We have, therefore, followed Whipple and Dallenbach in calling the problem, in this sort of experiment, the range of visual apprehension.

#### *Conclusions*

1. The observed relative frequencies of correct judgments in experiments upon range of visual apprehension follow a continuous function of ogive form
2. The statistical limen, that stimulus-value for which correct judgments are given in 50 *per cent.* of the cases, is the most reliable measure of the range of visual apprehension.
3. In our experiments the individual limens for simultaneous visual apprehension ranged from six to over eleven stimulus objects (dots).

## EMOTIONS AND INSTINCTS

By HENRY C. LINK

For many years the instincts have been identified, to a greater or lesser extent, with the emotions. In 1880, G. H. Schneider<sup>1</sup> gave the first developed expression of this identity. James soon after suggested the co-ordination between instinct and emotion in two successive chapters under these titles.<sup>2</sup> But the most ambitious attempt to establish this identity is that made by W. McDougall, who even couples specific emotions with specific reactions.<sup>3</sup> For example, he identifies the emotion of anger with the instinctive reactions of pugnacity, the emotion of fear with the instinct of flight, disgust with repulsion, wonder with the actions of curiosity, the tender emotion with the parental instinct, elation with self-asserting activities. He names other instincts and emotions such as gregariousness, the constructive instinct, and hunger; but these, he says, have no definite corresponding emotion.

The question which all of these views promote is the question as to the character of this identity. First of all, are the emotions and instinctive motor reactions identical in time? It is often stated that they are, and we are sometimes told that the emotions are but the subjective aspect of the instincts. However, the tendency is to regard either one or the other as prior. The James-Lange theory insisted upon the priority of the instinctive response and the subsequent presence of a characteristic emotion. James himself repudiated this view to a certain extent when he admitted that the perception of a total situation, and not a mere physical object, initiated both the emotion and the instinctive response.<sup>4</sup> The extensive experiments of Sherrington, described especially with reference to their bearing upon the James-Lange theory, led him to assert as his theory that the bodily changes and the psychic

<sup>1</sup> G. H. Schneider, *Der tierische Wille*, 66, 96, 1 and 6 ff.

<sup>2</sup> *Principles of Psychology*, vol. ii.

<sup>3</sup> *Social Psychology*, 45 ff.

<sup>4</sup> W. James, "The Physical Basis of Emotion," *Psychological Review*, i, 1894, 523 ff.



factor or the emotion occur concomitantly.<sup>5</sup> Sherrington also says that "of points where physiology and psychology touch, the place of one lies at emotion."<sup>6</sup> Further experiments, made by W. C. Cannon, confirm this theory. Cannon says: "We do not feel sorry because we cry, as James contended, but we cry because when we are sorry or overjoyed or violently angry . . . there are nervous discharges by sympathetic channels to various viscera, including the lachrymal glands."<sup>7</sup> That is, the two are simultaneous as far as time is concerned. In fact, it is difficult to see how one could be found to precede the other. In animals, for example, where, as Sherrington says,<sup>8</sup> we have to infer the character of the emotions from the physical manifestations, it is impossible to say whether one or the other comes first. In ourselves it is impossible to decide because, when our attention is turned to the active side of emotion, we are only secondarily aware of the emotion itself; whereas, when we are most conscious of the emotion, we are only secondarily aware of its motor accompaniments.

Another way in which instincts and emotions are identified is by defining them both as dependent upon the same bodily mechanisms. Each emotion has its corresponding visceral and organic activities and characteristic bodily expression. This, in its most extreme form, is the view held by McDougall, who says: "In the case of the powerful instincts, the affective quality of each instinctive process, and the sum of visceral and bodily changes in which it expresses itself, are peculiar and distinct."<sup>9</sup> However, the experiments of Sherrington and Cannon may be regarded as having disproved this theory. The view that the differentiated features of emotions are due to the viscera was discredited by Sherrington, when he demonstrated that emotional responses occur in dogs in which practically all the main viscera and the great bulk of skeletal muscles have been removed from subjection to and from influence upon the brain, by severance of the vagus and spinal cord. He admits that the visceral reactions strengthen the emotion; they are nevertheless of relatively small importance "as compared with the cerebral reverberations to which is adjunct the psychical component of emotional reaction."<sup>10</sup> In support of this view, W. B. Cannon says: "The evidence which I

<sup>5</sup> *Integrative Action of the Nervous System*, 258.

<sup>6</sup> *Op. cit.*, 256.

<sup>7</sup> W. B. Cannon, *Bodily Changes in Pain, Hunger, Fear, and Rage*, 280.

<sup>8</sup> *Op. cit.*, 266.

<sup>9</sup> *Social Psychology*, 46.

<sup>10</sup> *Op. cit.*, 268.

have given above, as well as that which he (Sherrington) has offered, favors the conclusion that the viscera are relatively unimportant in an emotional complex, especially in contributing differential factors."<sup>11</sup> On the other hand, d'Allonnes<sup>12</sup> points out, very shrewdly, that Sherrington's experiments would upset the peripheral theory only if we could prove that animals actually experience the emotions whose signs they manifest. He himself takes the opposite view, basing his conclusions upon observations on a woman who, having lost her visceral sensibilities of pain, hunger, thirst, and fatigue, exhibited all the signs of anger, fear, chagrin, etc., but at the same time asserted that she did not feel any of these emotions. In this case, the proof hinges upon whether or not we can believe what the subject said.

From the above testimony it must be apparent that the physiological method with animals can never demonstrate what connection exists between emotions and bodily activities. In the first place, it begins with the assumption that certain acts express certain emotions, which in itself is a questionable assumption. In the second place, it concludes that, whenever the characteristic motor expressions are present, as for example the retreat of a dog as if frightened, the emotion is also present, and whenever these motor expressions are absent, the emotion is also absent. Goltz, for example, made experiments upon dogs which showed that when the vascular and visceral fields had been severed from connection with the head, the animal was still able to perceive objects and react to them with the characteristic motor responses of joy, fear, excitement, etc., while the dog whose cerebral hemispheres had been removed was able to respond only to the affective situations of pain and pleasure.<sup>13</sup> What such experiments can prove at the most is that certain characteristic motor or organic reactions are dependent upon certain parts of the nervous or vasomotor system, but they cannot prove that those reactions are invariably connected with the emotions they are supposed to represent.<sup>14</sup>

Watson cuts through this difficulty by discarding, almost entirely, the traditional distinction between instinct and emotion. Instead of regarding emotion as the subjective or psychological aspect of instinct, he regards it simply as a slightly different type of motor response. "Emotion is an hereditary

<sup>11</sup> *Ibid.* 28.

<sup>12</sup> G. Requin d'Allonnes, *Les indications pour les dans la psychologie des animaux*, 1908, 228 ff.

<sup>13</sup> Sherrington, *op. cit.* 265, 266.

<sup>14</sup> *Op. cit.* 266. E. B. Titchener, *Text Book of Psychology*, 481 ff.



'pattern-reaction' involving profound changes in the bodily mechanism as a whole, but particularly of the visceral and glandular systems."<sup>15</sup> Whereas "instinct is an hereditary pattern-reaction, the separate elements of which are movements principally of the striped muscles."<sup>16</sup> Without discussing the refinements of these definitions we easily see that they indicate a radically different conception of the whole subject of emotion and instinct. By defining instinct and emotion both as somewhat different but essentially similar objective phenomena, Watson avoids, for all practical purposes, the many confusions and philosophical speculations which have characterized the studies dominated by the James-Lange point of view. Whether the differentiation he makes will prove valuable is another question. At least we have here a statement which recognizes the futility of the traditional views and sets out on a track which has a fair chance of being proved or disproved objectively.

It seems likely that the attempt to define and classify emotions through introspection can never become more than purely conjectural. The endless variety of opinion which we find among psychologists as to the character and number of the emotions is eloquent testimony to the futility of this procedure. It is the very nature of emotion to connect itself with almost any objective expression whatsoever, and hence, from a subjective point of view, it is hopeless to try to establish even relatively invariable connections between definite reactions and emotions. The tendency for emotions to become more and more diffuse, and to be identified with an ever-increasing range of objects and interests, but confirms this statement. James, although he associated instinctive responses with emotions, clearly recognized this difficulty when he said: "In short, every classification of emotion is seen to be true and natural as any other, if it only serves some purpose; and such a question as 'what is the "real" or typical expression of anger or fear?' is seen to have no objective meaning at all."<sup>17</sup>

In spite of this obvious problem some psychologists have presented classifications of emotions and ascribed to them objective phenomena with a completeness and certainty which is certainly not warranted by the facts. McDougall, for instance, begins with a certain number of innate instinctive emotional dispositions, and upon these as a basis constructs the entire fabric of life as it is found today. "The instinctive impulses," he says, "determine the ends of all activities and supply the

<sup>15</sup> *Psychology from the Standpoint of a Behaviorist*, 195.

<sup>16</sup> *Op. cit.*, 231.

<sup>17</sup> *Psychology*, 381. f.

driving power by which all mental activities are sustained; and all the complex intellectual apparatus of the most highly developed mind is but a means towards these ends, is but the instrument by which these impulses seek their satisfactions."<sup>18</sup> McDougall enumerates a group of primary instincts, about seven in number, each of which he claims is differentiated by a distinct emotional core and a characteristic bodily activity.<sup>19</sup> Upon these instincts, then, he proceeds to build the various institutions, customs, habits, and interests which characterize society. He traces to them the development of morals; of religion; of the higher sentiments of love, hate, and respect; of self-consciousness, volition, ideals; in short, of the whole range of life. A more thoroughgoing psychological rationalism it would be difficult to imagine. The very comprehensiveness of the scheme gives it an increased plausibility, for, having begun with the statement that the instincts are the factors which determine and sustain "all" the activities of life, he has not omitted to catalogue all those activities under some instinct or other.

In the first place, what is the principle by which McDougall distinguishes the primary emotions and instincts? "Each of the principal instincts," he says, "conditions some kind of emotional excitement whose quality is specific or peculiar to it; and the emotional excitement of specific quality that is the affective aspect of the operation of any one of the principal instincts may be called a primary emotion."<sup>20</sup> This principle, McDougall holds, is "of very great value when we seek to analyze the complex emotions into their primary constituents."<sup>21</sup> However, this is merely a postulate or an arbitrary statement of what McDougall intends that an instinct-emotion shall mean. Whether such entities exist, and how we can distinguish them if they do exist, is still the problem. McDougall suggests two principles which he thinks will be of great help in picking out the primary instincts. The first is that "if a similar emotion and impulse are clearly displayed in the instinctive activities of the higher animals, that fact will afford a strong presumption that the emotion and impulse in question are primary and simple."<sup>22</sup> The second principle of discovery is to inquire whether the emotions and instincts in question are subject to morbid hypertrophy or excitability.<sup>23</sup>

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<sup>18</sup> *Op. cit.*, 33.

<sup>19</sup> *Ibid.*, 45 ff.

<sup>20</sup> *Ibid.*, 47.

<sup>21</sup> *Ibid.*, 47.

<sup>22</sup> *Ibid.*, 49.

<sup>23</sup> *Ibid.*, 49.

With regard to the first of these criteria, the analysis presented above suggests the trend of our criticism. If, in the first place, we infer the presence and quality of emotions in animals by comparing their attitudes with our own, how can we legitimately infer the number and kind of our own primary emotions by referring them to the analogy which we have created? To create an object by introspection, and then to use it as an objective criterion by which to establish the primal quality of our own emotions, is indeed a vicious circle. Moreover, the claim that "in case of the powerful instincts, the affective quality of each instinctive process and the sum of visceral and bodily changes in which it expresses itself are peculiar and distinct"<sup>24</sup> is not borne out by the facts. Even if it were possible to take the higher animals and base a deduction of the emotions upon the instinctive activities which they manifest, it would still be impossible to draw this inference; for, as the above analysis shows, the motor reactions which characterize certain affective processes are dependent upon visceral disturbances in only a minor degree. As for the second criterion, it seems reasonable to think that if each instinctive emotion is a "relatively independent unity in the constitution of the mind," it would be liable to morbid hypertrophy or abnormal excitement. However, the forms of abnormality display as great a complexity as the forms of normal life, and hence do not offer that decisive individuality which the given definition of instinct would lead one to expect. And if it is difficult to connect typical acts with primary emotions in our own minds, and impossible to do more than conjecture the nature of the connection in animals, how much more difficult is it to connect the disarranged and abnormal activities of an abnormal person with any particular emotion? Psychiatry has succeeded in connecting ideas with ideas, events with events; but it has only partly suggested the exact nature of the connection between specific acts and specific emotions.

The choice of instinctive emotions which McDougall makes clearly bears out the above criticisms. For example, the gregarious instinct, according to McDougall, is not marked with an emotion "sufficiently intense or specific to have been given a name;"<sup>25</sup> nevertheless, he attributes to it the tremendous growth of cities and an immense range of social phenomena. The same may be said of the constructive instinct and the instinct of acquisition. Both of these are responsible, in McDougall's social structure, for a great deal, in spite of the fact that they are not marked by any specific emotion.

<sup>24</sup> *Ibid.*, 46.

<sup>25</sup> *Ibid.*, 86.

On the other hand, joy, which is thought to display itself in one of the most immediate and manifest responses in some of the higher animals,<sup>26</sup> is listed by McDougall among the sentiments. The instincts of subjection and elation which McDougall considers primary are obviously of a questionable character. They are so general that they may be interpreted into almost any situation. Finally, the principle which governs McDougall's choice of instincts is too susceptible to inversion. By that I mean that McDougall is more intent upon reading the facts back into instinct than upon reading the facts out of the instincts. Given a certain class of phenomena, he believes that there must be some instinct to account for it, and he never fails to find one appropriate. Strangely enough, McDougall severely criticizes this kind of procedure, giving as an example the statement made by V. Cousin as the fundamental assumption of his philosophy of history. This statement is as follows: "The various manifestations and phases of social life are all traced back to tendencies of human nature from which they spring, from five fundamental wants, each of which has corresponding to it a general idea. The idea of the useful gives rise to mathematical and physical science, industry and political economy; the idea of the just to civil society, the State, and jurisprudence; the idea of the beautiful to art; the idea of God to religion and worship; and the idea of truth in itself, in its highest degree and under its purest form, to philosophy. These ideas are argued to be simple and indecomposable, to co-exist in every mind, to constitute the whole foundation of humanity, and to follow in the order mentioned." McDougall then adds: "We have here the spectacle of a philosopher, who exerted a great influence on the thought of his own country, and who rightly conceived the relation of psychology to the social sciences, but who, in the absence of any adequate psychology, contents himself with concocting on the spur of the moment the most flimsy substitute for it in the form of these five assumptions."<sup>27</sup> McDougall's criticism is just. He himself has the advantage of a knowledge of comparative psychology, and hence has been able to identify man with the brute. This always creates a presumption in favor of the profundity of an analysis. However, as far as scientific coherence is concerned, McDougall's presentation may seem as fanciful to future psychologists as Cousin's statement does to present day philosophers. The criticism which Titchener makes of McDougall's pro-

<sup>26</sup> *Integrative Action of the Nervous System*, 265 f.

<sup>27</sup> *Op. cit.*, 12 f.



cedure is applicable here. He says that the list of instincts which McDougall presents is "a matter of individual preference rather than of scientific finality."<sup>28</sup> Titchener's criticism points out the root-fallacy in McDougall's work. The very nature of the situation is such as to make a scientific formulation of instinctive emotions impossible. And if it is impossible to distinguish the separate instincts and "driving" emotions, it is futile to attempt to trace to them the particular facts and values which now confront us. This is the more true because, as McDougall himself says,<sup>29</sup> the emotional centers, through their cognitive receptors, can come to enforce almost any particular motor expression or fact. Taking into consideration, therefore, the difficulties mentioned above, it is impossible to see to what practical result McDougall's list of instincts can lead. McDougall's confessed purpose is to make a contribution to the Social Sciences by analyzing the dynamic and determinative factors in human nature. However, he has left us with a mosaic of facts which are connected only in a schematic way. Beginning with a handful of supposedly specific, but in actuality hopelessly vague causes, he has left us with an infinitude of results, all obvious but all more or less irrelevant.

However, McDougall's analysis is admirable in its attempt to account for the dynamic nature of instinct by means of emotive forces. No matter what view is held about the number and kind of the emotions, the general opinion is that they are, as the word suggests, moving forces, "contributing not a little to the energy of behavior" (Morgan). Granting, for the time being, that there is a determinate number of instinct-emotions, and that they are the core around which all subsequent modifications and growth take place, we are led to ask: How, or by what principle, do these emotions combine to form the various purposes and values which we possess? How, in the event of a conflict between the various emotions, is the conflict resolved? Or, if the various emotions give rise to various values, all more or less independent, what will be the criterion of the 'relative' value of the desires which these emotions reinforce? McDougall states this problem and explains it by saying: "In the absence of sentiments our emotional life would be a mere chaos, without order, consistency, or continuity of any kind; and all our social relations and conduct, being based on the emotions and impulses, would be correspondingly chaotic, unpredictable, and unstable. It is

<sup>28</sup> *Textbook of Psychology*, 480.

<sup>29</sup> *Op. cit.*, 34 f.

only through the systematic organization of the emotional dispositions into sentiments that the volitional control of the immediate promptings of the emotions is rendered possible. Again, our judgment of values and of merit is rooted in our sentiments; and our moral principles have the same source, for they are formed by our judgments of moral values."<sup>30</sup> The problem which is vital to the discussion and which McDougall names only to ignore is this: How are the emotions organized into a system which shall thenceforth determine the manner in which those emotions shall express themselves? In answer to this question, we must say that in so far as McDougall regards the emotions as fixed and fundamental forces, he has made it impossible for them to create a principle by which they shall be subordinated. On the other hand, in so far as he admits that the instincts are subordinated and harmonized by some other force, or by other causes, he has introduced a principle which is in direct contradiction to his definition of the instincts.

The existence of some factors other than the particular emotions themselves, which determines how they shall express themselves, is generally recognized. It shows itself in the tendency, on the one hand, to analyze each emotion into elements more and more discrete and minute, namely, into sensation qualities; and, on the other hand, to reduce the separate emotions to two or three comprehensive emotions. All such explanations, whether they assume as fundamental a few general emotions, or analyze the general emotions into minute elements, show the same difficulty, namely, the impossibility of explaining the unity of the emotions in terms of units which, by definition, are discrete.

In pleasantness and unpleasantness, however, we have two factors which, in a sense, unify the various emotions by giving them a common character. All emotions are affectively characterized by pleasure or unpleasantness, and it is this common element which determines the direction of the emotion. It is often said that pleasure and unpleasantness serve to guide the emotions, rather than actively to direct or determine them. What it means to guide rather than to direct activity may be decided by those who insist that the man behind a flock of sheep is directing and driving the flock while the men on either side are merely guiding them. It is also very commonly held that pleasure and its opposite are merely signs indicating that the body is functioning normally or abnormally. This is to liken them to sign-posts or to static symbols between which

<sup>30</sup> *Social Psychology*, 159 f.

the emotions proceed upon their path. It is impossible to interpret the affective processes in this negative fashion without destroying their real significance. The reason, probably, why the affective aspect of consciousness has come to be regarded as merely a "guide" is because it is usually diffuse and is concomitant with almost every conscious state. This, however, is not a good reason for depriving feeling of its dynamic significance. If we regard the emotions as dynamic, we must also regard pleasure and unpleasantness as dynamic. In fact, it is quite likely that pleasure and unpleasantness are more fundamental than the emotions, and that not only genetically but actually. The experiments which Goltz performed on dogs, for example, showed that the most persistent and durable reactions were those induced by a stimulus calculated to produce pleasure or pain.<sup>31</sup> Genetically, it is likely that the emotions were subsequent to the more general affective process of pleasure and unpleasantness. However, we must take one more step and ask: What determines the nature of the affective response, whether it be pleasurable or the opposite? Is it some single emotion? Hardly, because then the word as we have been using it would become meaningless. An emotion which determines all other emotions could certainly not be an emotion in the sense in which we have been using the word. The text-books answer this question by saying in several ways the same thing, namely, that when the organism is getting along well or functioning normally, the pleasurable state predominates; while, when the organism is getting along poorly, the unpleasant state predominates. But what does this imply? It implies that there is an organism which has a normal course, a positive and dynamic character of some kind, or possibly a purpose; and when this normal course, whatever it may be, is thwarted, displeasure ensues. When it proceeds normally, pleasure ensues. That is to say, no particular emotion and no particular feeling is the fundamental dynamic factor in life.

### *Conclusions*

The classical concepts of emotion and instinct, represented in the controversies which center around the James-Lange theory, are a hindrance rather than a help to future studies in this field. By regarding emotion as a psychical phenomenon, and instincts as the bodily changes which characterize the emotion, it is made absolutely impossible to establish any causal identity, or even a schematic identity, between the two.

<sup>31</sup>*Integrative Action of the Nervous System*, 266.



Valuable objective studies, such as those of Goltz with dogs, are only made confusing by attempts to interpret into the bodily reactions the emotions which introspection has named. Therefore, the ambitious attempts on the part of some writers to identify instinctive responses with a group of emotions arbitrarily selected is open to serious criticism. And when the entire range of individual and social phenomena is attributed to such a group of uncertain causes, only the most fanciful and uncritical imagination can follow the process by which it is done. But even if we admit the validity of the assumption upon which such a superstructure rests, there is another logical difficulty which we encounter. If we assume, as does McDougall, that the emotions are the fundamental forces in the organism, and that these emotions are fixed in character, it becomes logically impossible to explain the process by which these conflicting forces are unified. How can independent forces give rise to a working principle or "sentiment" which is more powerful than the fundamental forces themselves? This difficulty is recognized in those views which seek to interpret all emotion in terms of the affective processes of pleasantness and unpleasantness. But the analysis of pleasantness and unpleasantness leads us again to the conclusion that even these affective processes derive their significance from some positive content of the organism itself, a content which the current theories of instinct and emotion have not yet exhausted. Whether or not we are able to give to this content any objective meaning is highly problematical. Our present purpose has rather been to point out certain contradictions and absurdities which characterize present thinking in this field, and particularly to call attention to the highly speculative character of the concoctions in which instinct-emotions are used as the prime ingredients.<sup>32</sup>

<sup>32</sup> Save for the paragraph on Watson's view, this paper was written some time before the appearance of Kantor's *A Functional Interpretation of Human Instincts* (*Psych. Rev.*, xxvii, 1920, 50ff.). With slight differences, the writer is in substantial accord with the conclusions reached by Kantor. Indeed, the similarity of the criticisms of McDougall is striking. Kantor, however, has succeeded the better in giving consistent expression to the functional point of view. The writer hopes soon to present his own conclusions in more comprehensive form.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF  
VASSAR COLLEGE.

XLI. THE RELATION OF THE PLEASANTNESS OF COLOR COMBINATIONS  
TO THAT OF THE COLORS SEEN SINGLY

By M. F. WASHBURN, DOROTHY HAIGHT, AND JEANETTE REGENSBURG.

Professor Geissler contributed to the Titchener Commemorative Volume a study of the affective tone of color combinations, the most positive result of which was to show that "the greater the pleasantness of the individual constituents, the greater will be the pleasantness of the combination." The materials used were the following saturated Bradley colors; red, orange, yellow, green, blue-green, blue, and purple. These were shown in areas of 5 cm. a side, placed side by side to make a strip 10 cm. wide. When single colors were to be judged, the left half of this strip was covered. The background was grey. The method was that of Paired Comparison; one color combination, or one single color, being shown 5 cm. above the other.

The design of our own experiments was to see whether the principle that the pleasantness of a color combination varies directly as that of its components would hold good when absolute rather than comparative judgments of affective value were made, and in general to see what light could be thrown on the problem by altering the method. Instead of the Method of Paired Comparison, we used the one which has been so often employed in our laboratory, that of presenting a single impression, either a color combination or a single color, and asking the observer to express his judgment of its pleasantness by using the number 1 to mean 'very unpleasant,' 2 'moderately unpleasant,' 3 'slightly unpleasant,' 4 'indifferent,' 5 'slightly pleasant,' 6 'moderately pleasant,' and 7 'very pleasant.' Another important difference between our method and Geissler's lay in our use of shades and tints as well as saturated colors. We used twelve saturated colors of the Bradley series, twenty-four shades, and twenty-eight tints. The colors were pieces 3 cm. square, pasted on white cards  $2\frac{1}{2}$  by 3 inches. Where color combinations were used, two of these squares were pasted side by side on the same card. Each of the two experimenters used a different set of colors, comprising thirty-two of the sixty-four single colors, which were shown and judged first, and sixteen combinations of these colors, which were shown and judged later in the same sitting. There were thus in all thirty-two combinations used. The number of observers, all women college students, was 105 for one experimenter, 106 for the other.

The relation between the pleasantness of the combinations and that of the single colors was found in the following way. The total affective value of every color, when shown alone, was determined by adding the numbers by which the observers expressed their

judgments of its pleasantness; the sum, of course, would be larger, the pleasanter the color was found by the observers as a group. Then the sums thus obtained for each of two colors that were shown in combination were added; the combined sum represented the combined affective value of these colors when they were shown singly. This being done for each of the pairs of colors, it was possible to arrange them in an order representing their pleasantness as determined solely by their appearance singly. Then the numbers which represented the affective judgments of the observers on the combinations as such were added, and the combinations arranged in the order of the size of these sums. Evidently the rank difference correlation between these two arrays will give the degree of relationship between the pleasantness of a color combination and the individual pleasantness of its component colors. This correlation proved to be plus .747, with a P.E. of .0119. It is clear that to a very considerable extent, the pleasantness of a color combination depends upon the pleasantness of the individual colors.

But this fact can hardly be taken, as Professor Geissler seems to take it, for a manifestation of the simple summation of feelings. If the agreeableness of a color combination is due to summation of the agreeableness of its components, or its disagreeableness to summation of the disagreeableness of its components, then a combination of two pleasant colors should be pleasanter than either of the components when seen singly; and a combination of two unpleasant colors should be unpleasanter than either of the components seen singly. This follows from the orthodox doctrine of the simplicity of feeling tones. Now in the entire series of our experiments, in which thirty-two combinations were used, with over a hundred observers judging each combination, there were 861 cases where the component colors, when judged singly, were both found agreeable. In 263 of these cases, or 30.5%, the combinations of these colors were found positively disagreeable. There were 465 cases where the component colors, judged separately, were both found disagreeable; of these 72, or 15.4%, were agreeable in combinations. These figures support the conclusion to which ordinary experience points, that *the unpleasantness or pleasantness of a color combination is derived not merely from summation of the affective tones of its components, but from another factor dependent on the combination itself.* Our results suggest also that this factor more often gives rise to unpleasantness than to pleasantness; that it is easier to get an ugly combination out of two pleasing colors than a pleasing combination out of two disagreeable colors. It may, however, be true that our thirty-two combinations happened to include more cases where the combination factor operated to produce unpleasantness. The relation of the two factors, that derived from the component colors which is responsible for our correlation of plus .74, and that derived from the combination, may be conjectured to depend on whether attention is attracted to the colors as separate colors, when they appear in the combination, or to the total effect.

## BOOK REVIEWS

*Lehrbuch der experimentellen Psychologie.* By J. FRÖBES, S.J. Herdersche Verlagshandlung, Freiburg. Bd. I, 1917, 605 pp.; Bd. II, 1920, 704 pp.

The first impression that one gets from this book is an impression of exceptional bulk; a royal octavo of 1,300 pages closely printed in large and small type, and with relatively few figures! The second impression is that of a wealth of citations from other works: the *Autorenverzeichnis* contains over 1,300 names (300 more than the fifth edition of Wundt's *Grundzüge*); and the author assures us, in the preface, that with very few exceptions he has quoted no book or monograph which he has not read through and judged for himself. Moreover, some of the writers are cited not once only, but many times; Wundt and G. E. Müller over a hundred, Fechner forty-six times; between these extremes come in descending order of frequency Lehmann, Meumann, Ebbinghaus, Titchener, Ribot, Stern and James, and Helmholtz. Our author has evidently not only read diligently, but has also chosen wisely. It was his aim, as he tells us again in the preface, to write a text-book of Psychology of the order of Tigerstedt's *Lehrbuch der Psychologie* or of Ebbinghaus' *Psychologie*. Furthermore, the book is designed to serve as an introduction not only to experimental psychology, as the title might lead us to suppose, but also to the other branches of psychology. Finally, the writer says that he has not found it necessary to give in the present book an exposition of his psychological faith. This he hopes to do in another place at some future time; in the meanwhile, the reader who is curious to know more on the point may find views similar to his own in Geyser's *Lehrbuch der allgemeinen Psychologie*.

The first volume of Fröbes' book is devoted to what he regards as the elementary psychological processes. It comprises a short introduction; two long sections, the one on sensation and feeling, the other on perception; and two shorter sections on psychophysics and on the association of ideas. The second volume is concerned with the 'higher mental activities.' It begins with a transitional section on cortical localization and derangement of associations. Then follow four long sections on *intellectual processes* (attention, the self, memory and learning, intelligence, creative imagination, and language); on *emotion* (emotion in general, classification of emotions, the aesthetic feelings, and expressive movements); on *will* (movement and reaction, the will-consciousness, custom and religion, and personality and mental development); and finally on *anomalies of consciousness* (sleep and dreams, hypnotism and suggestion, and mental pathology in general). Under some of these chapter-headings are materials of differential psychology, social psychology, educational psychology, juristic psychology and the psychology of aesthetics. An enormous programme! What does the author make of it?

He defines experimental psychology as the "science of mental processes, their laws, and their connection, as based upon observation and experiment." By the term 'mental processes' he means psychical

phenomena or phenomena of consciousness, the objects of inner experience. He dismisses definitions by point of view (of which he refers to Wundt's as an instance) solely by the assertion that physics and psychology have no objects in common. The problem of psychology is the description and explanation of the phenomena of consciousness; explanation, of sensation at least, is to be found in physical objects and their effect upon the nervous system. All this is said in the most summary fashion. Fröbes devotes, however, nine pages to method. He recognizes as methods of psychology introspection and observation of behavior (*Fremdbeobachtung*). His treatment of the former is mainly an abstract of the views of G. E. Müller; concerning the latter he follows in the main W. Stern. His notion of sensation is reminiscent of James, Höfler and Geyser: sensations are the elements of the more objective processes, the *Erkenntnisvorgänge*; they are items of knowledge of attributes which belong not to the knowing subject but to the sensory describable object; they combine to form perceptions; and they are also the elements which, at still higher stages, give rise to conceptions and judgments. Every sensation has four attributes, quality, intensity, temporal duration and spatial extension; the two latter, however, do not in Fröbes' thinking belong to sensation in the narrow sense; they are rather 'formal elements,' which assist in the formation of perceptions. Simple feelings, on the other hand, he regards as the elements of the 'more subjective processes' like Emotion and Will. They are dependent upon sensations, but are differentiated from them principally by virtue of their subjectivity. Fröbes also recognizes simple and complex ideas (*Vorstellungen*); the former he considers as 'renewals' (*Erneuerungen*) of earlier simple sensations; the latter are complexes of simple ideas, as perceptions are complexes of simple sensations. Although he discusses differences in intensity, in activity, in direction of attention (Fechner), and Müller's 'indistinctness' of normal images, as marks of differentiation between sensation and simple idea, he concludes that no one of them is essential. In all these systematic questions, however, one feels that the author is not seriously interested; the definitions seem to be merely introductory, purely formal, and are given as if they were to be expected and must be got out of the way. Moreover, the simpler processes are not inherently necessary to the higher. Attention is, for example, placed among the higher intellectual processes, of which sensations are by definition the elements; yet attention suddenly appears full-blown as clearness of higher processes; it is characterised as the opposite of distraction, of wool-gathering, and as subjective (not the objective clearness or distinctness of objects); after this characterisation the literature of attention is discussed under the familiar rubrics; and that is the end of attention. Again, when we come to emotion, the higher feelings are differentiated from the lower by difference in temporal course, in ease of inhibition, in dependence upon mental conditions, etc.; but the question how the simple combine into the higher feelings is never even raised. This lack of interest in writing a psychology that is systematic in any sense other than that of mere classification along conventional lines occasionally leads our author to unfortunate results. Among these is a failure at times to understand clearly the systematic views of other writers. For example, in his discussion of the attributes of sensation he says: "Titchener replaces the attribute of extent (*Räumlichkeit*) by that of clearness, which is determined by the strength of attention" (I, 28). Another result



is a confusion in classification. For instance, the section on thought (in which the Würzburg and the Cornell studies, Wundt's reaction against the thought-experiments, Koffka's book, etc., are reported) divides the chapter on perception into two parts; it occurs after the detailed accounts of the perceptions of space, time and movement, and before those of synaesthesia and the perception of form. The author gives no reason for this grouping; and whatever we may guess leads to the conclusion that the problem of perception has never been fully realized. What, then, it may be asked, is the author's positive contribution to our science?

The book is a collection of facts on and about psychology, drawn from a large number of different sources; the results of experiments conceived from various points of view, selected according to the compiler's judgment, classified under the usual chapter-headings, and moulded into readable form. The attempt is unique in psychology; the nearest approach, Myers' *Text-book of Experimental Psychology*, is still very different. The positive value of the book depends, therefore, upon the relative value of the facts selected, the completeness with which the ground is covered, the accuracy with which the facts are reported, the skill with which they are presented, and the aids supplied for the reader's convenience. From these points of view let us examine the book once more.

The section on sensation is, aside from that on association of ideas, the most complete of the entire work. The facts of visual sensation are taken principally from Hering and his pupils, v. Kries, Titchener, Ebbinghaus, and G. E. Müller. Helmholtz and König are, however, not ignored. The chief innovation is the extended incorporation of Müller's views, drawn not only from the well-known *Zeitschrift* articles but also from Fröbes' manuscript notes of Müller's lectures taken in 1903. The chapter on auditory sensation is based upon Stumpf, Helmholtz, and Schäfer's article in Nagel's *Handbuch*. Two pages are given to Révész' *Qualität*; and in the paragraph on *Klangfarbe* there is a report of the vowel-controversy, which includes the work of Köhler and of Jaensch; the solution of both these problems is, however, left to the future. The principal omission is that of tonal volume, which is mentioned only as a 'quantitative element' of *Klangfarbe*. On the side of theory we have only a brief account of Helmholtz', and a still shorter reference to that of Ewald; for others the reader is referred to Schäfer. Taste and smell, cutaneous, kin-aesthetic and static, and organic sensations are likewise reported from the standard sources, and on the whole are faithfully reported and clearly presented. Apparently, neither Henning's work on taste and smell nor Boring's on sensations of the alimentary canal was available; but, at any rate, are missing. The account of simple feeling is mainly a summary report first of the controversies about feeling (which suffers for want of insight into the reasons for the differences of opinion), and secondly of the results of experiments by the method of expression, particularly those of Lehmann and the Wundtians. All of the experimental work based upon the method of impression is ignored. The experimental work on 'simple ideas' is also insufficiently reported. In the long section on perception, the principal facts are taken from the important secondary sources like Hermann's and Nagel's *Handbücher*, the treatises of Wundt, Ebbinghaus, Stumpf, Witasek, etc. There are also many reports of original experiments and occasional abstracts of long books. One wonders at times at the relative amount of space assigned to various topics;

why, for instance, six pages should be given to a digest of Katz' *Erscheinungsweisen der Farben*, significant as that book is, and only about the same amount to the geometrical-optical illusions. Or again, one may miss a fact like Wertheimer's Phi-phenomenon which seems important enough for mention. On the whole, however, the section is well done. The point of view of the *Psychophysik* is that of Müller; and the treatment of methods, and discussions of the significance of the limen, of Weber's law, etc., are drawn principally from Fechner, Müller, Titchener, and Lehmann. The article is, however, brought up to date; the views of Wirth and Urban are presented; and the section closes with a long chapter on correlation. The best parts of the final section of the first volume, on association of ideas, are, as one might expect from a pupil of Müller's, those which deal with the methods and results of the association-experiments. The worst part is the chapter on ideational types, which is inadequate as regards the available facts. The association-reaction, including Jung's *Tatbestandsdiagnostik*, is described in this section.

This brief analysis of the first volume must suffice for the present review, although the second volume is a different book, and might well have had a different title; it is a 'general' psychology. And because under many chapter-headings there is no large body of accepted fact upon which to draw, there is greater opportunity for disagreement as regards the choice of the facts which go into these chapters. The author has, however, in most cases chosen his authorities wisely: Meumann and Müller for memory and learning, Wundt for language, Ach for will, Fechner, Groos, Witasek and Külpe for aesthetics, Stern for differential psychology, Preyer and Stern for child psychology, Kraepelin, Störring and Janet for abnormal psychology. If no one of the chapters based on such authorities would fully satisfy the expert, they will at least give the reader an orientation, not in the points of view of the various psychologists, but in the principal facts. And this, we suppose, is the author's aim. It must, however, be added that arrangement and emphasis often leave much to be desired: let the reader try, for instance, to determine from Fröbes' references the various usages and the preferred signification of the important term 'empathy'!

Taking the book as a whole, and overlooking minor errors and omissions, we must judge it as a well-selected compendium of the facts of psychology. So far as our reading has gone, we have found the reports of the principal facts reliable; the author is not so trustworthy in reciting controversies or in expounding systematic views because he has not taken the trouble to go beneath the controversies or sympathetically to understand systematic differences. He has, as he planned, written a book of the order of Tigerstedt's *Physiologie*, but he has not approached (as he also desired) Ebbinghaus' *Psychologie*. For Ebbinghaus had a point of view which he tried to drive through the *Grundsätze*; and however inadequate he may at times have been, he always had an insight which penetrated to the psychological significance of his facts, and he was at all times critical, keenly discriminative, and constructive. Fröbes, on the other hand, is a reporter who has read widely and painstakingly, but from the outside; if he attempts criticism, he is likely to be superficial; if he is discriminative, it is seen only in his selection of facts for treatment and not in his treatment of the facts; and, lastly, he is never constructive; he throws no new light upon old facts, opens no novel perspective, makes no new generalization.

H. P. W.



*Vorlesungen über die Menschen- und Tierseele.* By W. WUNDT. Sechste neubearbeitete Auflage mit 53 Figuren im Text. Leipzig, L. Voss, 1919. pp. xvi, 579. Price (at time of publication) Mk. 26.

This book has had an amazing career. It began in 1863 as a two-volume work whose programme was nothing less than the rounding-off of all departments of psychology, experimental, animal, social, into a systematic whole. Since in those early days Wundt had not learned to distinguish between psychology and applied logic,—he soon came to the discovery, and he never tired of proclaiming it,—there is no reason to wonder that his attempt was a failure. Critics declared, and the Wundt of 1906 is not unwilling to agree, that the book so far from inaugurating a reformation in psychology, was merely an immature and misdirected essay.

In course of time, nevertheless, the edition was exhausted; and in the late eighties the original publisher came to Wundt with the suggestion of a new issue. Wundt hesitated for some time, but finally acceded to the request. There was, he thought, a certain amount of the old matter that might still stand, and there was a vigor and freshness of style that he might not easily recover. The book was, also, something of an incubus; the printed word, however emphatically he should elsewhere renounce his change of view, bound him to statements that he would gladly revoke; and a new edition would formally supersede the earlier work. So a single volume appeared, as a second edition, in 1892. The logical framework was discarded; the lectures on social psychology were excluded; the whole text was rearranged and recast. This is the edition that Creighton and I translated in 1894—the first of Wundt's books to be published in English.

A third German edition was called for in 1897, and a fourth in 1906. The fourth edition is remarkable on the ground that Wundt here tried, for the first time, to do rough justice to the modern developments of animal psychology. The incident is eminently characteristic: Wundt, who knew nothing at first hand of the lower forms of life save what he had learned—how many years before!—in the zootomic laboratory of Johannes Müller, and who knew nothing at first hand of the higher forms save what he had learned by intercourse with the domestic cat and dog, undertakes at the age of seventy-four to review the 'literature' of animal psychology, to pass critical judgment upon it in the light of human psychology, to bring out the fundamental questions of principle, to show connections, to mark lacunae, all for the sake of three or four chapters of a popular book. It is needless to say that these chapters, and the preface that goes with them, inadequate as they must appear to the professional student of animal behavior, are nevertheless of high value to psychologists at large; they give us animal psychology in the Wundtian setting; they show us the relation in which Wundt placed animal psychology to individual human psychology no less clearly than the encyclopaedic *Völkerpsychologie* shows the relation of social psychology to the same central discipline.

The new chapters were popular; a fifth edition came out in 1911; and a sixth and final edition—let us hope that no editor rises up to lay hands upon it—in 1919. There is little that is new in this last revision: a note that brings the physical theory of relativity into connection with the psychological is, perhaps, the most important addition; all changes are minor. The book remains what it was in 1892, "a pretty mixed medley of new and old," but a medley so

cunningly mixed that the layman will never suspect the diversity of origin, and a medley so various that the psychologist may find in it an epitome of the history of modern psychology. It is a long stretch from 1858 to 1919, a long stretch especially in the history of a young and rapidly growing science. Wundt has been able, as no one else can ever be, to compress a vast deal of this history, for the benefit of those who read with understanding, into the limits of a single volume.

E. B. T.

*Fugitive Essays.* By J. ROYCE. With an Introduction by J. Loewenberg. Cambridge, Harvard University Press, 1920. 429 pp.

Here are printed fifteen of Royce's essays. All of those previously published were virtually inaccessible; all but three belong to the period 1878-1882; and four appear in print for the first time—"The Practical Significance of Pessimism" (1879), "Tests of Right and Wrong" (1880), "On Purpose in Thought" (1880), and "Natural Rights and Spinoza's Essay on Liberty" (1880). The editor contributes an illuminating introduction (37 pp.), in which he emphasises the continuity of Royce's earlier and later work. The volume contains a good portrait of Royce; it should have had an index.

*Collected Essays and Reviews.* By W. JAMES. New York, Longmans, Green & Co., 1920. pp. x (Preface by R. B. Perry), 516.  
*Annotated Bibliography of the Writings of William James.* By R. B. PERRY. New York, Longmans, Green & Co., 1920. pp. ii, 69.

All psychologists will be grateful to Professor Perry for the labor of love that he has spent on these two books. The first contains thirty-nine scattered articles and reviews written by the late Professor James between the years 1869 and 1910. They include the "Remarks on Spencer's Definition of Mind as Correspondence" (1878), "The Sentiment of Rationality" (1879), "What is an Emotion?" (1884), "The Original Datum of Space-Consciousness" (1893), and many another classical paper. The Bibliography, based upon James' and Holt's List of 1911, comprises (if a hurried counting may be trusted) 311 titles, arranged chronologically from 1867 to 1920. Most of the titles receive brief comment, and an alphabetical index is appended.

*Social Scandinavia in the Viking Age.* By MARY W. WILLIAMS. New York, The Macmillan Co., 1920. pp. xiv, 451, with 50 illustrations and map. Price \$6.00.

This is an interesting and scholarly book—undoubtedly the best introduction to its subject that we have in English. It meets fairly the charge of our childhood's histories that the Northman, admirable in courage, was at the same time drunken and licentious, an inveterate gambler, a violator of his oath. After an introductory sketch of the land and the people, the chapters take up in order the ties of kinship and nationality; the classes of society; infancy, childhood and youth, dress, ornament, personal refinement; marriage and divorce; the position of women; homesteads and houses; house-furnishings and food; agriculture and the routine of farm life; hunting, fowling, fishing; internal travel, ships and nautical science; trade and commerce; markets and towns; the career of the Viking; his weapons and warfare; government; system of justice; social gatherings, recrea-

tions, amusements; language and literature, the runes; learning in general; scientific knowledge, art; objects of worship; places and methods of worship; superstition; death and burial. The illustrations are well chosen, and the book ends with a useful bibliography and a full index.

*The Philosophy of Don Hasdai Crescas.* By M. WAXMAN. New York, Columbia University Press, 1920. pp. xii, 162. Price \$1.75 net.

Hasdai Crescas, the last of the Jewish mediaeval thinkers, was born of a noble and wealthy Catalanian family in 1340, lived the greater part of his life at Barcelona, and died at Saragossa in 1410. He wrote a work entitled "The Light of God," in which among other things he criticised Maimonides' proofs of the existence of God, and his views of the divine attributes and of the relation of God to the world, and set forth his own positive convictions. In the present study Dr. Waxman expounds the system of Crescas, whom he exhibits as an intellectual rebel against Aristotle, a probable influence upon Spinoza, an analyst surpassing Maimonides himself. The Introduction deals with Jewish and Arabic philosophy at large, with the treatment of the problem of the existence of God in pre-Maimonidean Jewish philosophy, and with the theology of Aristotle. Part I then discusses the problem of God, his existence, essence, unity, attributes; and Part II the relation of God to man, as exemplified in the questions of free-will and determinism, providence, immortality. The study concludes with bibliography and index.

The following books have been received:

- L. KLAGES. *Vom Wesen des Bewusstseins, aus einer lebenswissenschaftlichen Vorlesung.* Leipzig, J. A. Barth, 1921. Pp. vi, 94. Price Mk. 12.
- G HEYMANS. *Einführung in die Metaphysik auf Grundlage der Erfahrung.* Dritte durchgesehene und vermehrte Auflage. Leipzig, J. A. Barth, 1921. Pp. vi, 364. Price Mk. 56.
- F. WELTSCH. *Gnade und Freiheit.* München, Kurt Wolff Verlag, 1920. 157 pp.
- J. B. PRATT. *The Religious Consciousness, a Psychological Study.* New York, The Macmillan Co., 1920. Pp. x, 486.
- E. GALLI. *Alle Radici della Morale.* Roma, "Unitas," 1919. 415 pp.
- E. GALLI. *Nel Domino dell' "Io."* Roma, "Unitas," 1919. 205 pp.

## NOTES

Wilhelm Wundt died at Leipzig on the afternoon of Tuesday, August 31, 1920, a fortnight after the celebration of his eighty-eighth birthday. Psychologists of all interests and of all shades of opinion unite to do homage to the memory of the foremost representative of their science. We hope in a later number to print a sketch of Wundt's life and psychological work.

Théodore Flournoy, who held the chair of psychology and history and philosophy of science at the University of Geneva, died on Nov. 5, 1920, at the age of 66. Flournoy published in 1893 a volume on *Des phénomènes de synopsie*, and became widely known by his *Des Indes à la planète Mars: étude sur un cas de somnambulisme avec glossolalie*, 1900 (translated into English in the same year). He published also: *Le génie religieux*, 1904; *La philosophie de William James*, 1911 (translated 1917); *Esprits et médiums*, 1911 (English abridgment, *Spiritism and Psychology*); *Une mystique moderne*, in *Arch. de psychol.*, xv, 1915; *Métaphysique et psychologie*, 1919 (reprint, with preface by H. Höffding, of the edition of 1890); and many other studies in the *Archives* and elsewhere. He was co-editor with Professor Claparède of the *Archives de psychologie* since the foundation of that journal in 1902.

Alexius von Meinong, professor of philosophy in the University of Graz, died on Nov. 27, 1920, at the age of 67. Meinong is best known by his contributions to the border-discipline which he named *Gegenstandstheorie*; his *Gesammelte Abhandlungen* were published in three volumes in 1913-14. His doctrine of supposals (*Ueber Annahmen*, 1902, 1910) attracted widespread attention. Meinong also wrote on topics of direct interest to experimental psychology (Weber's law, the color-pyramid, etc.), and in 1894 founded at Graz the first Austrian laboratory, from which has come a long and valuable series of experimental studies. He was strongly influenced by Franz Brentano, but with his friend Alois Höfler arrived at conclusions which Brentano vigorously combatted; indeed, we owe to Meinong much of the new matter in Brentano's *Von der Klassifikation der psychischen Phänomene* (1911).

Elmer Ernest Southard, professor of neuropathology in the Harvard Medical School, from 1912 to 1919 director of the psychopathic department of the Boston State Hospital, and in 1919 appointed director of the Massachusetts State Psychiatric Institute, died Feb. 8, 1920, at the age of 44. The Bulletin of the Massachusetts Department of Mental Diseases, iv, no. 1, Feb., 1920, which is issued as a Southard Memorial number, contains a characteristic portrait, an appreciation by Dr. R. C. Cabot, and a bibliography.



## SYSTEMATIC PSYCHOLOGY

Professor Stumpf has recently published in the Memoirs of the Prussian Academy of Sciences, and also as separate works, two important monographs in systematic psychology.<sup>1</sup> (1) The first (1917) deals with the attributes of visual sensations. Aside from spatial and temporal characters, which he does not consider, Stumpf recognizes three attributes: quality, brightness, and intensity. The change which differentiates the progression of blue-red-yellow from the progression black-grey-white is a change of quality; the change which is common to the two progressions is a change of brightness. The achromatic series thus shows, like the color series, changes both of quality and of brightness. Intensity, the outstanding difficulty of the psychology of visual sensation, receives thorough discussion. Stumpf frankly accepts the attribute, on the analogy of the other senses, and seeks evidence for it. He finds in the grey (which, in popular speech, is a dark grey) of the resting eye—G. E. Müller's middle grey, Révész's critical grey—the weakest visual sensation, and in direct sunlight the strongest. Theoretically, all the visual qualities, including of course the blacks which are darker than the critical grey, lie in a single straight line of intensity between these terminal points. As a matter of fact, the qualities that we see in ordinary diffuse daylight are of approximately constant intensity. If, however, we observe strongly illuminated (especially yellow) surfaces on a dark background we may note definite intensive changes: the yellow, *e. g.*, remains the same yellow, but as the illumination increases becomes yellower, not in the sense of greater saturation, but in the intensive sense of more strongly yellow. These observations, Stumpf argues, are sufficient to establish the attribute of intensity: collateral evidence is found in the experiments upon color limens.—Saturation or chroma then necessarily disappears as a visual attribute. Stumpf declines to recognize insistence, and apparently declines to recognize clearness or vividness, though formally he leaves this issue open.

(2) In the second monograph (1918) Stumpf seeks to determine the difference between sensory and imaginal experience, between *Empfindung* and *Vorstellung*. He begins by discussing, with special reference to tones, the criteria currently applied: presence and absence of external causes, specific diversity of contents, specific diversity of acts, gradual difference of intensity. The evidence points decisively to a mere difference of degree. Stumpf accordingly adopts this position, and from it rebuts a number of objections: that, with a merely intensive difference between sensation and image, we could not talk of an imaginal fortissimo; that metric comparisons of the intensity of sensations and images ought to be possible; that images could not be present simultaneously with sensations of the same sense; and that the facts of the limen would be unintelligible. He then turns from hearing to sight, and considers the intensity of visual images, their spatial properties, the phenomena in the neighborhood of the limen, and the separation of colors and greys in ordinary ideas and in hallucinations.

<sup>1</sup> "Die Attribute der Gesichtsempfindungen," *Abh. d. kgl. preuss. Akad. d. Wiss.*, Jahrgang 1917, phil.-hist. Klasse, no. 8. Einzelausgabe, Berlin, 1917; price (at the time of issue), Mk. 5.25. 88 pp.

"Empfindung und Vorstellung," *Abh.*, etc., Jahrgang 1918, phil.-hist. Klasse, no. 1. Einzelausgabe, Berlin, 1918; price (at time of issue), Mk. 6.75. 116 pp.

The upshot is that all *sinnlich-anschauliche Erscheinungen*, all the 'sensory' and 'imaginal' experiences, of a determinate kind form a single intensive series, ranging continuously from the weakest 'image' to the strongest 'sensation.' From the purely phenomenological point of view there is no reason for a distinction of the two classes. We gradually learn, however, that experiences of a certain range of intensity are usually due to outside causes, and we thus come in course of time to an immediate, unreflective differentiation of perception from idea. Scientifically, the line of division is drawn at the stimulus-limen. But once the dividing line has been drawn, once the scale of intensities has been calibrated, a conscious reference-to-object is unnecessary; henceforth intensities above the limen go by the very fact of their intensive rank to the class of sensations, intensities below the limen to the class of images.

Stumpf proceeds to the enumeration of secondary criteria. *Vorstellungen* are poorer than *Empfindungen* in immanent and concomitant characters; they are fleeting, or at any rate less sharply delimited in duration; they are largely modifiable at will. And, as a result, the affective influence of *Vorstellungen* is on the average less than that of *Empfindungen*; and "if the question of the real significance of the phenomena arises, it is in the case of *Vorstellungen* bound up with the consciousness that the belief in reality needs justification, while in face of *Empfindungen* this belief is, at least for the naive consciousness, immediate."

If all images lie below the intensity of the stimulus-limen, it follows that many experiences which we currently number among images must be renamed sensations; Stumpf devotes a section to subjective sensations and hallucinations. From these he passes to the collateral evidence for the qualitative likeness of sensation and image: associative memory, the fusion of reproduced and perceptive elements in the unitary empirical object, associative reproduction and the recognition of purely imaginal formations, likeness of sensory and motor effects. Finally, he distinguishes between images of memory and images of recollection, but hesitates to recognize a class of images of imagination.—

Whether or not we agree with Stumpf's conclusions, we may be heartily grateful to him for the patient thoroughness with which he has worked them out; and fortunately they may, in most cases, be put to the ultimate test of experiment. The brightness-attribute of visual sensations will, one is disposed to think, find ready acceptance; the intensive attribute will doubtless be disputed, but has at any rate achieved respectability. Saturation may be dispensed with the more easily if one accepts Dimmick's view of grey as not the specific mid-term of a single black-white series and the end-term of chromatic series, but the end-term of six specifically qualitative series. As regards sensation and image, I confess that Stumpf's exaltation of intensity does not so far convince me, and that his account of the observer's behavior in the liminal region does not square with my own experience. I incline rather to a differentiation by 'body,' by collocation of attributes. Here as elsewhere, however, experiment will decide.

E. B. T.

## THE AMERICAN PSYCHOLOGICAL ASSOCIATION

The twenty-ninth annual meeting of the American Psychological Association was held at the University of Chicago, December 28, 29, 30, 1920, with approximately one hundred fifty members present. The Sections of Psychology and Education of the American Association for the Advancement of Science met at the same time and in the same building and members of both organizations circulated freely between parallel programs. The fifty-seven papers presented before the American Psychological Association were about equally divided between experimental and general psychology on the one hand and applied psychology and mental tests on the other. After the annual banquet the retiring President, Dr. Franz, gave the presidential address on "Cerebral-Mental Relations," in which he brought together the recent evidence against fixed localization of function in the brain. Messrs. Cattell, Judd, Scott, and Pintner spoke informally in appreciation of Wilhelm Wundt, presenting personal reminiscences of their work with him.

The meetings were marked by considerable discussion of plans of organization in psychology and of the ways and means for meeting the demands which the recent extensions of psychology have placed upon scientific sources of fact and personnel. There was also evidence, as numerous papers attested, of a desire on the part of experimentalists to break down distinctions existing at present between schools of psychology and to emphasize psychological community of thought rather than differences of points of view.

The election of officers resulted as follows: President: Margaret Floy Washburn, Vassar College; Members of the Council, 1921-1923: George F. Arps, Ohio State University; Walter S. Hunter, University of Kansas; Nominees for appointment to the Division of Anthropology and Psychology of the National Research Council: Walter B. Pillsbury, University of Michigan; George M. Stratton, University of California.

E. G. B.

## RECOGNITION OF FACES

The prompt and sure identification of persons by recognition of their faces is so obviously a matter of practical importance that it is amazing that it has not earlier become an object of critical and experimental study. Dr. Henning<sup>1</sup> has, however, laid an excellent foundation in analysing the problem and working out a method for a quantitative attack upon it, from which follow in natural sequence a method for the training of those whose business it is to recognise faces, and a new field for testing in vocational guidance. Incidentally he has also supplied psychology with a striking instance of the operation of the "apperceptive mass," and furnished ethnology with a new method of racial classification.

For the details the paper itself must be consulted; but it may be noted that unerring identification of faces is dependent upon the analysis of them, *i.e.*, upon attention to certain primary facial elements (eyes, eye-brows, nose, mouth, etc.) and their variations—

<sup>1</sup> H. Henning, Das Wiedererkennen menschlicher Gesichter in kriminologischer Hinsicht. Mit 14 Abbildungen. *Arch. f. Kriminologie*, 72, August, 1920, pp. 235-254.



inexpert looking at faces is always satisfied with unanalysed total effects—and that the method consists in finding the minimal facial area, determined on a photograph or a real face by means of an Aubert diaphragm or something similar, which permits certain recognition.

The illustrations show the bilateral asymmetry characteristic of most faces and the difficulty of recognising even well-known faces when seen in part only. It is regrettable that the pictures illustrating bilateral asymmetry complicate the matter by introducing a gross and unnecessary difference in the arrangement of the hair.

E. C. S.

#### A FURTHER WORD ON SUPERSTITIOUSNESS

Since publishing my study of the superstitions of college students (this JOURNAL, 30, 1919, 83), I have discovered a somewhat similar study made over thirty years before and published under the rather misleading title of "First Report of the Committee on Experimental Psychology" (*Proc. of American Society for Psychical Research*, 1, No. 3, 1887). The report is signed by the committee's chairman, Professor C. S. Minot. A form of questionnaire was used, and the study was designed to "test the prevalence of a tendency to superstition in the community." The three questions in the list upon which the conclusions of the report were based sought the frequency of tendency to superstitiousness concerning the number thirteen, Friday, and seeing the new moon over the left shoulder. Why these three—and only these were selected is not made clear. Five hundred returns were tabulated, but no definite statement appears in the report concerning the class of people from whom the returns came, except a vague statement in the concluding paragraph about "the educated portion of our community" and a reference to New England.

This report concludes that 10% of men and 20% of women have a tendency to superstition, and the committee expressed surprise at the large number. My own results indicate a much larger percentage of both men and women (men admitting present belief or practice 40%, and women 66%). Can it be that there has been such an astonishing increase in superstitiousness in thirty years? Probably not. The small figures which the committee obtained are more likely due to the curious limitation of their study to three superstitions. It is true that the committee's returns were from adults chiefly, while mine were entirely from adolescents; but an analysis of their tables indicates that the returns which they included from those of adolescent years present the same small percentages.

Isolation of the committee's returns from adolescents, for comparison with my own returns, indicates that the committee found about 11% of young men with a superstitious tendency and about 17% of young women. My results for the same age-group were 40% and 66%, approximately the same ratio. That two studies made thirty years apart, in localities three thousand miles apart, and by somewhat different methods, should indicate the same degree of difference between the sexes adds much to the reliability of the conclusion.

Neither Dresslar's work nor mine, however, supports the conclusion of the Minot committee that there is a greater tendency to be superstitious about the new moon than about either Friday or the

number thirteen. As our combined work covers so many more cases, it must certainly be that the Minot committee were led astray by a chance result

University of Oregon.

EDMUND S. CONKLIN.

#### COMBINATIONAL TONES REGISTERED BY THE TONOSCOPE.

In a recent number of this JOURNAL<sup>1</sup> the writer reports the effectiveness of the tonoscope for registering difference-tones of the first order. Later experiments justify further conclusions.

In this study difference-tones of higher orders were investigated. For this purpose the generators employed were the bugle and the voice. Two points were essential in selecting the range of the generating notes: that their difference-tones should lie within the limits of the tonoscope-readings, or at least not beyond a very small multiple of the readings, and that the various difference-tones should be far enough away from one another and from their generating notes to prevent overlapping and blurring of the dots on the tonoscope. When these precautions were observed, records of difference-tones of the second and third orders were as easily and clearly obtained as those of the first order had been in the earlier experiments.

BUGLE TONE		SINGING TONE		Tonoscope Readings for Difference Tones in vibs.
Approximate note	Tonoscope Readings in vibs.	Approximate note	Tonoscope Readings in vibs.	
a# <sup>1</sup>	120 × 4=480 160 × 3=480	f# <sup>1</sup>	123 × 3=369 184 × 2=368	D <sub>1</sub> 111 × 1=111 D <sub>2</sub> 129 × 2=258 D <sub>3</sub> 147 × 1=147

Records for summation-tones obtained from these generators were then attempted without success.

Many theories have been advanced regarding the nature and origin of summation-tones. W. Preyer,<sup>2</sup> following a suggestion made by G. Appunn, explains them as differential tones derived from the action of partials rather than from the generating tones themselves. He declares that all cases of summational tones on record might conceivably fit into this theory. This view suggested a means of approach through the tonoscope. If, for example, the summation-tone of 500 vibs. arising from two notes with vibration-rates of 300 and 200 respectively may be regarded, not as the sum of the two generators, but as the difference between the first generator's second partial of 600 vibs. and the first difference-tone of 100 vibs., then generators possessing a strong second partial ought to register this 'summation' tone as easily as difference-tones, and generators lacking the second partial in any strength ought to fail to register this tone although difference-tones be clearly recorded.

To check this supposition trombones were selected because of their strong second partials, and clarinets because this partial is an almost

<sup>1</sup> E. Gough and G. Robison, The Tonoscope as a Means for Registering Combination Tones, *Amer. J. Psychol.*, 31, 1920, 91ff.

<sup>2</sup> H. Helmholtz, Sensations of Tone; Additions by the Translator, A. J. Ellis, 1895, 532.

negligible component of the tone.<sup>3</sup> Difference-tones of the first two orders were attempted with good results from both instruments.<sup>4</sup> The two trombones gave a clear reading of a summation-tone from the following generating notes: f, 176 vibs. and A, 117 vibs. The tonoscope recorded  $147 \times 2$ , or 294 vibs.<sup>5</sup> The clarinets were uniformly unsuccessful in producing readings for summation-tones, although conditions were well adapted for recording them if present.

More delicate and more extensive work needs to be done to investigate this theory further. These experiments tend to support the conception that summation-tones are to be regarded more accurately as difference-tones dependent on the presence of a strong second partial tone.

Smith College.

EVELYN GOUGH.

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#### APPOINTMENTS

Dr. Samuel W. Fernberger, recently Assistant Professor of Experimental Psychology at Clark University, has been appointed Assistant Professor of Psychology at the University of Pennsylvania. Mr. C. C. Pratt has been appointed instructor in experimental psychology at Clark University.

Dr. F. L. Wells has left the McLean Hospital, Waverley, Mass., to become head of the Psychological Department at the Psychopathic Hospital, Boston, Mass.

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<sup>3</sup> The writer was exceedingly indebted to the generosity and the patience of Northampton musicians in carrying on this section of the work. Mr. Carl Dodds and Dr. C. E. Perry played the trombones and Miss Myrna Wilderson and Mr. Carl Brand the clarinets.

<sup>4</sup> The clarinet gave good readings only for tones produced by closing most of the keys. Otherwise the air escaped through these openings instead of being directed into the tonoscope. The trombone proved an excellent instrument for this use.

<sup>5</sup> The second partial itself could never be read obviously from the tonoscope.

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## WILHELM WUNDT

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By E. B. TITCHENER

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Wilhelm Max Wundt was born, the son of a Lutheran pastor, at Neckarau in Baden, on August 16th, 1832. We know nothing of the family life of his parents, and nothing of his schooldays, though we may guess that he attended the Gymnasium at the neighboring town of Mannheim, of which in later years (1907) he became an honorary citizen.<sup>1</sup> The biographies begin with the statement that he spent the years 1851 to 1856 at the universities of Tübingen (where his uncle was professor of anatomy), Heidelberg and Berlin. His interest, at any rate for the greater part of his studentship, lay not in physiology (though he worked for a while in Johannes Müller's Institute) but in the purely medical subject of pathological anatomy. In 1855-6 he was assistant in the Medical Clinic at Heidelberg, and his *Inaugural-Abhandlung* (Untersuchungen über das Verhalten der Nerven in entzündeten und degenerierten Zuständen) is dedicated to C. E.

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<sup>1</sup> It is difficult to secure these details. I am not even sure of Wundt's middle name: the authorities here accessible give it as Max, but I seem to remember having seen it printed Maximilian. Nor do I know if Wundt had brothers and sisters.

Neckarau is a small place lying close to Mannheim, with which I believe it is now incorporated. Mannheim and Heidelberg are themselves only half-an-hour apart by rail.

Hasse, its director.<sup>2</sup> The biographies inform us, next, that he became Privatdozent at Heidelberg in 1857. His titular subject, whether as a matter of choice or of academic accident, was physiology.<sup>3</sup> He remained in this position, working for some years as assistant to Helmholtz, who came to Heidelberg in 1858,<sup>4</sup> until 1864, when he was appointed *extraordinarius*. Again there was a wait; Wundt's apprenticeship to the academic career was longer even than Kant's. In 1874, however, he received a call to Zurich, to the chair of inductive philosophy founded by F. A. Lange; and in the following year he was made a professor of philosophy at Leipzig.<sup>5</sup> Here he lived and worked for forty-five years,—rector of the university in 1889 (only fourteen years after he had joined the philosophical faculty), honorary citizen of the town in 1902, orator of the university at its five-hundred-year jubilee in 1909,<sup>6</sup> professor until 1917; and near by, at Grossbothen, he

<sup>2</sup> Wundt held the doctorates of medicine, philosophy and law. The doctorate of law was conferred upon him, *honoris causa*, by the University of Göttingen, in 1905. This year was the fiftieth anniversary of his doctorate of medicine, which was accordingly taken in 1855. The Heidelberg thesis bears the date 1856. (My copy has no *Vita*; but as the plate at the end is duplicated the *Vita*-leaf may have been omitted.) Did it serve both for *Dissertation* and for *Habilitationsschrift*?—I do not know where or when the doctorate in philosophy was taken; I have been told that it was an honorary degree.

<sup>3</sup> See title-page of *Die Lehre von der Muskelbewegung*, 1858 (Preface, 1857): a book dedicated to E. du Bois-Reymond, from whom and from whose pupils Wundt was presently to suffer sadly.

<sup>4</sup> The relations of Wundt and Helmholtz have not, to my knowledge, been thoroughly worked out. Personally, tradition says, the two men were uncongenial; and that would not be surprising, since their training was similar and their gifts and temperaments most dissimilar. But they speak of each other with mutual respect in the *Physiologische Optik* (1856, 1860, 1866) and the *Theorie der Sinneswahrnehmung* (1858-1862). When Helmholtz went to Berlin in 1871 his chair fell not to Wundt but to W. Kühne.

<sup>5</sup> His chief opponent was A. Horwicz. G. S. Hall tells us (*Founders of Modern Psychology*, MCMXII, 311) that the scale was turned in Wundt's favor by the local Herbartians. It must, surely, have been for them a choice of evils! For, if they had every reason to dislike Horwicz, they could still hardly have been much impressed by the preface to the *Physiologische Psychologie*.

<sup>6</sup> I have no list of Wundt's public honors. In 1911 he received the order *Four de mérite*, one of the most highly prized of European distinctions (30 German and 30 foreign members); and he was knight of various, I suppose Saxon, orders. He also became a *wirklicher Geheimrat* of Saxony, and was addressed as *Excellenz*.—It may be mentioned in passing that Wundt once attempted politics. In 1866 he was chosen representative of Heidelberg in the Baden second chamber. He very soon resigned.



died, on August 31st, 1920, a fortnight after his eighty-eighth birthday.<sup>7</sup>

Outwardly, then, Wundt's life was as uneventful as could well be: seventeen years at Heidelberg, and forty-five at Leipzig, with the Swiss interlude of a single year between. We have now to see what he made of this scholar's life; and we turn, naturally, to his books.

The book of primary importance for our purpose is the *Beiträge zur Theorie der Sinneswahrnehmung* of 1862. We do not know what led Wundt to the problem of perception: perhaps the Kantian atmosphere that he had breathed in Müller's laboratory,<sup>8</sup> perhaps the cases of anaesthesia that he met with in the Medical Clinic at Heidelberg, perhaps the discovery of a kindred spirit in E. H. Weber. At all events he writes a full-blown theory of perception, tactual and visual, four years before Helmholtz issues the third part of the *Optik*. We need not, however, concern ourselves with the psychology either of this book or of the more comprehensive *Vorlesungen über die Menschen- und Thierseele* which appeared in the year following. It was the psychology of the student of clinical medicine, of the biological technologist, made up out of general knowledge and common sense and medical case-histories as occasion required; we have plenty of it with us today, without needing to explore the work of half-a-century ago. What is of solid and enduring interest is the thirty-page introduction, *Ueber die Methoden in der Psychologie*, in which Wundt sets forth three ideas of first-rate importance: the idea of an experimental psychology, the idea of a social psychology, and the idea of a scientific metaphysics.

(1) If psychology is to advance, Wundt says, it must follow the inductive path. Two inductive methods are available: the method of statistics and the method of experiment. The former is an indirect method, since it bears primarily upon practical and not upon theoretical psychology. It brings with it, nevertheless, an extension of psychological observation; it furnishes psychology with new facts, guaranteed by the law

<sup>7</sup> Wundt's war-utterances we can only try to forget. We may be glad that he suffered no personal loss and (as it appears) no considerable personal discomfort during the troubled years; that he was able to work steadily to the completion of the *Völkerpsychologie*; and that he saw his son established as the successor of R. Eucken in Jena (W. Wirth, *Arch. f. d. ges. Psych.*, XL., 1920, xvi.).

<sup>8</sup> Kant and Herbart were the influences against which Wundt had to fight most continuously. They were accordingly the influences which most strongly affected him.

of large numbers; and in so far it is related to the direct method of experiment. This second method, Wundt declares, is in principle applicable over the whole range of general psychology. There is no hint of the restriction with which we later become familiar.<sup>9</sup> But neither is there, so far as I can see, any hint that the use of experiment is to safeguard the procedure and assure the results of that *Selbstbeobachtung* with which all psychology begins. Observation seems to remain pretty much what it had always been; only, by varying the conditions of observation, Wundt hopes to vary the mind's response to external stimuli and thus presently to arrive at laws of the mental life as such.<sup>10</sup> Not, I think, until 1881 did he express the modern view that "die exacte Beschreibung der Thatsachen des Bewusstseins . . . das einzige Ziel der experimentellen Psychologie [ist]."<sup>11</sup>

Whence, now, did Wundt derive his idea of an experimental psychology? I have no wish to belittle his originality; if I had, the attempt to do so would be futile. Ideas of this sort, however, do not spring readymade from the thought of an individual. And I believe that the proximate source of Wundt's idea is patent. No one can read the introduction to the *Beiträge* without being reminded of the sixth book of John Mill's *Logic*; and no one, I think, who after such reminder compares the two compositions can doubt that Mill, for whom psychology is explicitly a science of observation and experiment,<sup>12</sup> gave the cue both for Wundt's emphasis on improvement in method and for the concrete means to improvement, statistics and experiment, which Wundt pro-

<sup>9</sup> "Man ist häufig der Ansicht gewesen, gerade im Gebiet der Empfindung und Wahrnehmung [Ebbinghaus was nearly a quarter of a century in the future!] sei die Anwendung der experimentellen Methode noch möglich, . . . dagegen sei es ein vergeblicher Versuch, auch in das Bereich der höheren Seelenthätigkeiten auf experimentellem Wege vordringen zu wollen. Sicherlich ist dies ein Vorurtheil" (*Beiträge*, xxvii.).

<sup>10</sup> *Ibid.*, xxix.

<sup>11</sup> Ueber psychologische Methoden, *Philos. Stud.*, i., (1881) 1883, 3. The statement is sharpened in 1888. "Selbstbeobachtung [in the technical sense of *Beobachtung*] ist ausführbar, sie ist es aber nur unter der Bedingung der experimentellen Beobachtung" (Selbstbeobachtung und innere Wahrnehmung, *Philos. Stud.*, iv., 301). Cf. the appraisal of reaction-experiments in 1894: "Der Hauptwerth . . . dieser Versuche besteht . . . darin, dass sie die psychischen Vorgänge exact geregelten Bedingungen unterwerfen und auf solche Weise eine genaue Analyse der in der Selbstbeobachtung gegebenen Erscheinungen möglich machen" (Zur Beurtheilung der zusammengesetzten Reactionen, *Philos. Stud.*, x., 498).

<sup>12</sup> J. S. Mill, *A System of Logic*, etc., bk. vi., ch. iv., § 2; ch. v., § 5 (ii., 1856, 426, 447).



pounds. There is marked difference, over and above the cardinal difference that Mill talked about experiments and Wundt carried them out; but I have no doubt of Wundt's indebtedness to Mill.<sup>13</sup>

(2) The idea of a social psychology was in the German air at the time of Wundt's writing. In 1859-60 M. Lazarus and H. Steinthal had published the first volume of their *Zeitschrift für Völkerpsychologie und Sprachwissenschaft*, and their elaborate programme gave Wundt something positive to react against. We must follow the course of this reaction through several books.

In the *Beiträge* social psychology appears as an auxiliary science. General psychology must not only be improved methodically from within but must also be supported from without; and the supporting disciplines are two: first, developmental psychology, the psychology of the child; and secondly comparative psychology, the psychology of the lower animals and of human societies.<sup>14</sup> The preface to the first volume of the *Vorlesungen* of 1863 contains the sentence: 'Wo das absichtliche Experiment aufhört, da hat die Geschichte für den Psychologen experimentirt.'<sup>15</sup> This seems to look more directly toward the future: only, when we read the preface to the second volume, we find that the chapters in which experiment cannot be applied are those concerned with feeling, desire and action! Ethnological enquiry replaces experiment for the construction of a general theory of feeling; anthropology and the natural history of the lower animals give us an insight into instinctive actions, which leads on to a theory of the will; and the development of language serves to confirm psychological conclusions regarding the development both of feeling and of cognition.<sup>16</sup> We are wholly within the confines of general or individual psychology. Indeed, in a later note Wundt declares expressly that he has not, in the *Vorlesungen*, entered on the field of *Völkerpsychologie* as understood by Herbart, by Lazarus and Steinthal, and by Waitz.<sup>17</sup> In this book, then, we find—what is not infrequent in Wundt's

<sup>13</sup> Mill's *Logic* appeared in 1843, and the first German translation in 1849. See, e. g., *Beiträge*, 441. In the *Psychologismus und Logizismus* of 1910 (*Kleine Schriften*, i., 523) Wundt dates the first German edition 1862. Is this an evidential *lapsus*,—or did Wundt simply take the date from the second edition that he had used (1881) for his article on Mathematical Induction?

<sup>14</sup> *Beiträge*, xiv. f. The three departments of *Völkerpsychologie* are Sprachkunde, Culturgeschichte and Sittengeschichte.

<sup>15</sup> *Vorlesungen*, i., 1863, ix.

<sup>16</sup> *Ibid.*, ii., 1863, iii. f.

<sup>17</sup> *Ibid.*, 452.

work—a positive statement side by side with a flatly negative reaction to its immediate excitant.

The first three editions of the *Physiologische Psychologie* do not take us much further. In 1874 social psychology is essentially a descriptive, as opposed to an explanatory science. It has to do with complex phenomena, which must be illuminated by the laws of the individual consciousness; its task is largely classificatory.<sup>18</sup> In 1887 psychology is divided into (1) subjective psychology, which relies wholly on inner perception, and (2) objective psychology, which attempts to perfect and to supplement inner perception by objective means. Objective psychology, again, divides into (a) experimental or physiological psychology, which brings inner perception under the control of experimental appliances, and (b) social psychology, which seeks to derive general laws of psychological development from the objective products of the collective mind, from language, myth and custom. Formally, therefore, experimental and social psychology are co-ordinate and complementary. Materially, they are also mutually dependent for the collective mental life everywhere points back to the mental capacities of the individuals that make up the society, and the individual consciousness, especially in its more highly developed modes, is supported (*getragen*) by the mental life of the community.<sup>19</sup>

In all this there is nothing distinctively Wundtian. An even the essay of 1888 confines itself to a justification of the choice of language, myth and custom as the subject-matter of social psychology, and to the drawing of a cautiously qualified parallel between these three topics and the idea, feeling and will of the individual consciousness.<sup>20</sup> Not until 1893 are experimental psychology and social psychology "the two main branches of scientific psychology." Now, at last, we reach the peculiarly Wundtian position that experiment breaks down on the far side of perception and memory, and thenceforth the psychological system must be built up by way of *Völkerpsychologie*.<sup>21</sup> It is clear that, in the matter of experimental psychology, Wundt knew from the first what he

<sup>18</sup> *PP*, 1874, 4 f. So i., 1880, 4, except that the determination of the task is omitted.

<sup>19</sup> *PP*, i., 1887, 5 f.

<sup>20</sup> Ueber Ziele und Wege der Völkerpsychologie, *Philos. Stud.*, i., 1888, 20, 25 f.

<sup>21</sup> "Glücklicherweise fügt es sich jedoch, dass gerade da, wo die experimentelle Methode versagt, andere Hilfsmittel von objectiver Werthe der Psychologie ihre Dienste zur Verfügung stellen:" *PP*, 1893, 5.

was about, and modified his attitude only as his own psychological growth proceeded; whereas, in the matter of social psychology, he swung between different opinions, and reached his final standpoint only after a long course of trial and error. The difficulties in the one instance were mainly external; in the other, internal. Realising this, we shall give him all the more credit for keeping the troublesome subject of social psychology continually in mind.

(3) The third idea of the introduction to the *Beiträge* is the idea of a scientific metaphysics, a philosophy which makes the results of all the other sciences the object of its own special investigations.<sup>22</sup> To prepare himself for constructive work in the light of this idea, Wundt wrote, after the *Psychologie* of 1874, his *Logik* (1880) and his *Ethik* (1886). The *Logik* falls into two parts: *Erkenntnislehre* and *Methodenlehre*. The former, strictly logical part is at any rate competently done; the book takes its place with the best logical treatises of its generation. Its value pales, however, before the lustre of the *Methodenlehre*, a work that is absolutely without peer. Wundt's occupation with physiology had brought him familiarity with mathematics and the procedures of the exact sciences; his study of psychology had made him equally familiar with the methods of the mental sciences.<sup>23</sup> The result of this "encyclopaedic and round of knowledge" is a book that would of itself alone set its author in the front rank of contemporary thinkers. The *Ethik* deals, in four parts, with the facts of the moral life, the development of theories of the universe, the principles of morality, and the departments of the moral life. The characteristic feature of the work is, again, its scientific tendency, its attempt to derive the principles of morality from an empirical survey of the facts of moral living.<sup>24</sup>

After this manifold preparation Wundt went about the writing of his *System der Philosophie* (1889). The question had been, of course, whether the thing could be done; whether a full compass of scientific knowledge had not ceased to be possible, if not with Aristotle, at any rate with da Vinci;

<sup>22</sup> *Beiträge*, xiii.

<sup>23</sup> It has been said that the biological chapter falls below the standard of the others. I cannot agree. We have to remember the status of biology at the time when the chapter was written, and we have also (whether we like it or not) to presuppose Wundt's view of teleology.

<sup>24</sup> I confess that I have never felt at home with the Law of the Heterogony of Ends. It seems likely, if one pushes it far enough, to run sheerly counter to any ethical equivalent of the law of sufficient reason.

whether a modern, even if he had written a Physiology, a Medical Physics, a Psychology, a Logic and an Ethics, could rise on their basis to a genuine philosophy. Wundt replied by doing the thing in question. He draws up a complete programme of scientific philosophy, in every line of which he keeps his touch with science;<sup>25</sup> and he propounds a system in which no problem of that programme is shirked. We may accept or reject: Wundt has proved that this way of philosophising is still feasible.<sup>26</sup>

With the publication of the *System* it might well appear that Wundt had fulfilled his circle. He was fifty-seven years old; and he had enough to do, it would seem, in the revision of former texts (for all the larger books, the *Vorlesungen*, the *Physiologische Psychologie*, the *Logik*, the *Ethik*, the *System* itself, were going into new editions) and in the preparation, collection and revision of minor works (*Grundriss der Psychologie*, 1896; *Einleitung in die Philosophie*, 1901; *Essays*, [1885] 1906; *Kleine Schriften*, 1910-11; *Einführung in die Psychologie*, 1911).<sup>27</sup> As a matter of fact, he began forthwith to plan the largest of all his books, a book which causes us to retrace the path which we have too hastily been following: the ten-volume *Völkerpsychologie*, whose dates run from 1900 to 1920. The title-pages of the completed work still carry the familiar legend *Sprache, Mythos und Sitte*; but the plan grew with execution and revision,—Wundt's readers again demanded new editions; and the contents of the successive volumes are now distinguished as Language (2), Art, Myth and Religion (3), Society (2), Law, and Civilisation and History.

It is needless to lay stress on the intellectual vigor of a man who begins the publication of a work of this magnitude when he is sixty-eight, and continues its production over a period of twenty years. It is needless also to inform the JOURNAL's readers that Wundt's reputation has not suffered, has rather

<sup>25</sup> "Eintheilung der wissenschaftlichen Philosophie," *System*, 1889, 33 ff.

<sup>26</sup> The one large logical flaw of the *System* is the acceptance of the *Idee des letzten Weltgrundes*. Wundt honestly shows us his hand: "abweichend von allen anderen Vernunftideen ist dieselbe nämlich nicht durch einen directen Regressus von der Erfahrung aus erhalten worden, sondern nur infolge der allgemeinen Forderung, dass zu dem im Fortschritt der geistigen Entwicklungen sich vorbereitenden idealen Enderfolg ein dem letzteren vollständig adäquater Grund hinzugedacht werde" (439). He was himself subject to influences, historical and personal, which we who read him may not feel.

<sup>27</sup> A bibliography of Wundt's scientific writings will be found in this JOURNAL, vols. xix. (1908) ff.



(if possible) been enhanced, by his last achievement. I wish, however, to linger a little over the *Völkerpsychologie* in order to protest against a belief, current in recent years and in some measure encouraged by Wundt himself,<sup>28</sup> which I take to be grounded at best in a half-truth. A legend has grown up—I cannot call it anything else—to the effect that social psychology was Wundt's first and fondest love, and that all his life, up to about 1890, was spent in clearing intruders out of the way, that he might ultimately return to it. In part, the long stretch of years devoted to the *Völkerpsychologie* may be responsible; in part, as I have just said, certain statements of Wundt's own, made in what appears to be unnecessary self-defence;<sup>29</sup> in part, perhaps, a misunderstanding of the part played by social psychology in the early *Vorlesungen*, which are naturally more talked about than read. I should not accept this legend if it came with Wundt's own subscription; I should mistrust an old man's memory. I do not think that anyone can accept it who knows intimately the course of Wundt's development as his books portray it. At the beginning and for many years social psychology was rather for Wundt, as I called it above, a troublesome subject.

The kernel of truth in the legend is that Wundt was always attracted by troublesome subjects of a certain sort, subjects offering a certain type of data and inviting a certain kind of method. All of the major books bear a like stamp; they round up an incomplete and scattered subject-matter into tentative union and completeness; they are anticipations of system. They all, therefore, have about them a temporary and provisional air; they seem to promise new editions, to warn the reader that they will presently change. The preface to the first edition of the *Physiologische Psychologie* strikes the key-note: "die Orientierung über den Thatbestand einer . . . im Entstehen begriffenen Wissenschaft ist ja bekanntlich das beste Mittel, die noch vorhandenen Lücken zu entdecken." That note recurs, with such changes rung on it as the nature of the case demands, in every preface that Wundt wrote, from the *Vorlesungen* to the *Völkerpsychologie*. "Man kann möglicherweise zweifeln," he says of the *System*, "ob es angemessen sei, für eine derartige Untersuchung den alten Namen der Metaphysik zu wählen;" it is a new systematisation that he is attempting, the exposition of things

<sup>28</sup> E. g., in the preface to *Die Sprache*, 1900.

<sup>29</sup> Wirth, if I understand him aright, thinks that Wundt found Fechner's controversial insistence troublesome, and was a little afraid of getting side-tracked by applied mathematics: *Arch. f. d. ges. Psych.*, xl, 1920, xii. ff. Wirth, I take it, is also on the defensive.

from an unaccustomed point of view. Wundt is an essayist, only that his topics are not items but fields of knowledge.<sup>30</sup> It is small wonder, then, that—psychologist as he always was—he should be disquieted by the status and haunted by the problems of *Völkerpsychologie*, and should rejoice at last to bring psychological order into that chaos. But this is not to say what the legend says.

The twofold character of Wundt's work, as at once systematic and provisional, is a source both of strength and of weakness. It is obviously a good thing, if you are laying a case before the public, to think it steadily through, to view it in relations, to state it whole; so the argument becomes not only more impressive but also easier to grasp. It is a good thing, if you rely upon observations of fact, to sweep all your facts together, to organise them within a logical framework; so you become aware of support in unexpected quarters as well as of gaps that further work must fill. It is a necessary thing, if you are a man of science, to keep your ideas fluid, to let your theories sit lightly on you, to be open-minded toward new facts, to hold obstinately fast to nothing save the scientific point of view. But these good and necessary things imply a balance, and the balance of system and try-out, of system and first attempt, is not easy to maintain. Wundt was perpetually changing his evidence of observed fact and his minor perspectives; he expected to change them; the early data were but approximate and his first organisation of them must reflect their faults.<sup>31</sup> In so far he was plastic

<sup>30</sup> This view of Wundt's work is substantially the same as that taken by E. Meumann in the appreciation written for Wundt's eightieth birthday (*Deutsche Rundschau*, clii, 1912, 217 f., 220 ff.). Meumann and I roomed together during my second year at Leipzig, and by dint of endless discussion and reference succeeded in pigeon-holing Wundt to our satisfaction.

<sup>31</sup> Critics have made Wundt's readiness to change a ground of complaint; he changed his views surreptitiously, they say, without warning the reader or giving due credit to the men who forced the change. In so far as this charge implies moral obliquity on Wundt's part, it is ridiculous; Wundt, as all who knew him will testify and as his whole public career shows, was as honest as the day. Where he found a positive reason for noting change, he could be meticulously definite: witness the second edition of the *Vorlesungen*. Usually he thought it enough to assure his readers that he had taken the task of revision seriously, that the new edition was an edition and not a reprint, and to give a bare indication of the chapters most affected.

There is, however, no smoke without fire; and the critics in question are, I think, in fact objecting to a temperamental trait of Wundt's, his natural mode of reaction to criticism and suggestion. Külpe, with whom I once talked this matter over, pointed out to me that Wundt's development was always a development from within; his immediate



and receptive to an uncommon degree, and at an age when most men have settled down to fixed opinion. He did not either hesitate to throw overboard large theoretical constructions that his riper thinking disapproved; there is a great gulf between the *Beiträge* and the *Vorlesungen* on the one hand and the *Physiologische Psychologie* on the other. Yet he succumbed, without any doubt, to the temptations of the system. After 1874 (to take a rough dating) he showed little inclination to discard or revise his conceptual schemata; what had once been mere scaffolding thus tended to become an integral part of the actual building; or, to vary the figure again, Wundt poured the new wine of his later thought into the old bottles that he had more or less hurriedly assembled for his first successful vintage. I know, from many conversations, that he held his theories far more loosely than his readers ordinarily suppose, and that his greatest reverence was for fact. Yet it remains true that, when he had erected a theory, on however scant a basis of fact, he seemed as if in honor bound to defend it in his subsequent work. The theory might be changed contentwise out of all recognition; formally, nevertheless, it remained the original theory.

Had Wundt himself been aware that he was moving farther and farther away from his conceptual starting-points he would, with his indefatigable industry, have set about the task of revision. He was in fact aware, I imagine, rather of the continuity of his thinking; the later views seemed to him to be straightforward developments of the earlier, and therefore to be capable of expression in the same general terms. This sort of logical *Seelenblindheit* has had two regrettable consequences. The one is that Wundt was exposed to a hostile criticism which, as blind as he to the real issue, aimed only at the external and superficial, and which he accordingly and

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reaction to external suggestion was likely to be negative, but the new idea stayed with him, was incubated, and presently—perhaps long after—emerged with fresh coloring, in a novel context, variously modified, as a component of his own thinking. There were two consequences, which critics might very well find irritating. The first was that Wundt might read into early utterances of his own a pregnancy that they did not in truth possess; and the second (I have given an instance in the text) was that a positive statement might stand beside a negative criticism of the pre-Wundtian view which had, to all appearances, given occasion for the modified Wundtian formula. Other circumstances, social and professional, would possibly have made Wundt both more accessible and less sensitive to outside influence. But seventeen years of depression, followed by a rapid rise to a position that may almost be called pontifical, naturally served to harden his temperamental tendencies.

properly resented; the other is that students of Wundt must read his books in series, and can never hope to understand him fully from any single presentation of his thought.

We came to this discussion by way of the *Völkerpsychologie*. Retracing our steps still further, we arrive again at the first of the three ideas of the *Beiträge*, the idea of an experimental psychology. What Wundt made of this idea, so far as results go, all the world knows; what obstacles he had to overcome, and with what fortitude and persistence he overcame them, we shall probably never know.

In 1874 appeared the *Grundzüge der physiologischen Psychologie*, Wundt's most influential work. Beginning as a single-volume book, it grew to two volumes in the editions of 1880, 1887, 1893, and to three volumes in those of 1902-03 and 1908-11. In the first edition Wundt's psychology is in many ways crude; but it is nevertheless psychology, and not the applied logic of the *Beiträge* and the *Vorlesungen*; Wundt has struck his gait.<sup>32</sup> The controlling influences of his career were evidently operative between his thirty-first and forty-second years, though it is difficult to make out what they were. Perhaps the forthcoming autobiographical *Erlebtes und Erkanntes* will inform us.

Meanwhile we get no help from the list of publications. Wundt was busy, during the critical period, with his *Physiology* (1865, 1868, 1873); with *Die physikalischen Axiome* (1866); with the *Medical Physics* (1867); with the first part of his *Mechanics of Nerve* (1871). There is only a solitary article of 1867 entitled *Neue Leistungen auf dem Gebiete der physiologischen Psychologie*. And when he comes to write the *Physiologische Psychologie*, he relies for his physiological chapters, to be sure, on the work of these years of transition, but for his psychological data he goes back primarily to the *Beiträge* and secondarily to the *Vorlesungen*. No doubt he was maturing, fulfilling his normal inward growth. I think it a safe guess, however, that a strong negative influence emanated from Helmholtz, the final parts of whose *Optik* were issued

<sup>32</sup> The *Jubiläums Katalog der Verlagsbuchhandlung Wilhelm Engelmann in Leipzig* (i., 1911, facing p. 90) contains a facsimile of the letter in which Wundt offered the manuscript of the *Physiologische Psychologie* to the firm for publication. The letter is dated Decr. 8, 1872, and suggests that printing may begin in Feb. of the following year. Wundt outlines the work in five parts: the physiological properties of the nervous system, the doctrine of sensation and idea, the doctrine of organic movements, criticism of psychological doctrines, and general theory of psychophysical occurrence.

in 1866. Even the outward form of the *Psychologie* seems to indicate a certain feeling of rivalry.

Be that as it may, the *Physiologische Psychologie* was accomplished. Throughout the first four editions Wundt tried to keep it encyclopaedic, to make it a handbook of experimental psychology at large; the third edition is the best of these four. In the fifth and sixth editions he gave up that attempt, and frankly set forth his own psychological system. The change coincides with the ending of the *Philosophische Studien* and the founding of Meumann's *Archiv*, and its result is rather to maintain than to alter the status of the book. Wundt's laboratory had long been the heart and centre of psychological production; now the laboratories had multiplied.<sup>33</sup>

Here, then, is Wundt's first achievement in the domain of his experimental psychology. We can hardly overestimate it. As a work of reference the *Physiologische Psychologie* has been invaluable; its mere bulk and solidity have been an asset to a struggling science; the labor spent upon its revision has advantaged us all. But a greater achievement was to come.

In 1879—so runs the line in the biographies—Wundt founded the first psychological laboratory. We may let the bare line stand, if only it stand in lapidary letters. For that foundation was a world-event; it determined the very fabric and texture of modern psychology. Where John Mill theorised, Wundt performed; and the spirit of his performance has spread over the civilised world.<sup>34</sup>

Lastly, in 1881 Wundt began the publication of a magazine, *Philosophische Studien*,<sup>35</sup> the last two of whose twenty volumes (1902) constitute a *Festschrift* prepared by his former students for the occasion of his seventieth birthday. Unwearied

<sup>33</sup> In 1902 the *Zeitschrift* had reached only its thirtieth volume, and the division of the two *Abteilungen* was still ten volumes away.

<sup>34</sup> I knew, in my Leipzig days, something in detail of the difficulties that Wundt had to encounter. I wish I could trust my memory to rehearse them. I recall that strenuous objection was made to the new laboratory on the ground that continued self-observation would drive young persons to insanity! Instead of that, the success of the novel enterprise moved older persons to imitation. Wundt was fortunate enough to gain, in 1891, a colleague like-minded with himself,—the historian K. Lamprecht; and Wundt and Lamprecht together are primarily responsible for the development at Leipzig of those *Forschungsinstitute* that are a legitimate source of pride to the university.

<sup>35</sup> Wundt has more than once rationalized the title of this publication. The author of *Ein Druckfehler bei Kant* might refer to the preface of the *Vorlesungen*, where Wundt remarks "dass die philosophischen Studien nur in den Erfahrungswissenschaften den Boden einer fruchtbringenden Entwicklung finden können."



as ever, he started a new series, *Psychologische Studien*, in 1905,<sup>86</sup> and carried it through ten volumes to his retirement from the Leipzig chair in 1917. These two *Studien*-sets have an individuality that will always mark them off from other psychological periodicals. In the earlier, we see experimental psychology in the making; problems at first are few, methods are imperfect, mode of presentation is uncertain, perspective is almost lacking. But there is a steady growth, extensive and intensive; a professional attitude forms; and when we reach the *Festschrift* we find topics from the whole range of psychology—physiological and philosophical, normal and abnormal, individual and social, current and historical—competently and fruitfully handled in the Wundtian way. The later volumes present a different picture. Here we see the specifically Leipzig problems attacked with the utmost refinements of Leipzig technique. The *Philosophische Studien* thus have the attraction of *eine im Entstehen begriffenen Wissenschaft*; the *Psychologische Studien* have the fascination of expert workmanship in a single style.

It was, of course, a physical impossibility for Wundt, at his advanced age, personally to oversee the details of the experimental work carried out in his institute; W. Wirth was appointed co-director in 1908. But Wundt's editorship of the *Studien* was never perfunctory, and his interest in experimental psychology was always vigorous. In 1898 he was experimenting with the geometrical-optical illusions. In 1902-3—*Die Sprache* appeared in 1900—he was, for the first time, overtly systematising his general or individual psychology. In 1906 he upheld the sensory character of black. In 1907 he launched his attack upon the methods of the Würzburg school: in the interest, truly, of his own social-psychological theory, but in the most intimate terms of laboratory experimentation. In 1908 he published the first volume of the new *Physiologische Psychologie*, whose ninth chapter bears witness to an extraordinary resurgence of interest in the fundamental problems of psychophysics. In 1909 he discussed the issue of pure and applied psychology. In 1911 he revised and republished the *Psychologie und Naturwissenschaft* of 1903. As late as 1914 he wrote about the illusions of reversible perspective. Surely, there is no gainsaying this evidence! The

<sup>86</sup> Meumann's *Archiv* was first issued in 1903, overlapping the eighteenth volume of the *Studien*. Wundt was one of its cooperating editors, and agreed to publish in it the studies from the Leipzig laboratory. For the reason stated in his *Vorwort*—Wirth (*op. cit.*, ii) gives it as "die damalige glückliche Lage des deutschen Buchhandels"—Wundt preferred to recur to an organ of his own.

*Völkerpsychologie*, if I may repeat what has been said above, is a typically Wundtian book, an anticipatory system on the grand scale; it is the resolute outcome of a long period of perplexity; it furnished a grateful occupation for his old age; it is a work of exceeding value. But the dominant idea of Wundt's life, the idea upon which his reputation is most solidly based, the idea that persisted with him up to the very end of his university activity, is the idea of an experimental psychology.

And in a footnote to this list of his services in behalf of the idea, let us remember that Wundt was the first psychologist to bring demonstrational apparatus into the lecture-room. In his earliest Leipzig lectures he exhibited instruments and went through the motions of experiment. Very soon, however, he came to see the real purpose of a demonstration: the provision, namely, of conditions under which the audience may observe for themselves the fundamental phenomena of the subject-matter of discussion. His use of the lantern with illusions of reversible perspective, a brief account of which he published in 1907, is a very model of demonstrational procedure.<sup>37</sup>

It is plain that Wundt, whatever his intellectual gifts, could not have compassed this bulk of scientific work had he not been dowered with a good physical constitution and had he not lived a strictly regulated life. His days passed, in fact, with the regularity of clockwork. The morning was spent upon the current book or article; then came the *Sprechstunde*. The afternoon was taken up with the formal visit to the laboratory, a walk, the lecture, and a second, informal return to the laboratory. The evening was variously employed; Wundt might listen to a reader, or attend a concert or opera, or receive a group of his colleagues. For all his immersion in science, he managed remarkably to 'keep up' with current movements of literature and art. In personal intercourse he was unassuming, cordial, tolerant; by no means given to monologue; showing frequent flashes of a pleasant, wholly academic

<sup>37</sup> Wundt exerted a great and ever increasing influence as a lecturer. His habit was to throw his ideas into shape during his afternoon constitutional, and to speak without notes, though he always had a rough scheme of the topic in his pocket. I remember an occasion when his memory played him false in the matter of the name of a minor Greek philosopher; he extracted the paper of notes, and scanned it while still talking; but the notes, too, left him in the lurch, and the philosopher for that day went unmentioned. This is the only time that I saw him to refer in lecture to any written aid; and he remarked afterward that the experience had not been encouraging.

humor. There was no trace, as one sat with him in his own study, of the roaring lion of controversy or the somewhat Olympian arbiter of science and philosophy. He disliked public ceremonies, and could not be persuaded even to attend a psychological congress, though when occasion demanded his public appearance he played his part with dignity and success. He also disliked travelling, and his holiday excursions never took him far afield. These reluctances undoubtedly narrowed the sphere of his acquaintance, and so perhaps of his personal influence; but when the influence was already world-wide, when everybody who was interested in the things of mind came sooner or later to Leipzig, and when a greater *Geselligkeit* would have meant loss of productive time, they did not after all much matter. Wundt lived the simple family life of the old south-west German tradition, a retiring, sheltered life, which was probably the one condition under which his tremendous self-appointed task could have been accomplished.

As to the ultimate significance of that task, it would be the part of wisdom to keep silence; we stand too near to Wundt to see him in a just perspective. But I have formed my judgment, and will state it for what it may be worth. I take Wundt to be the first great figure in the history of thought whose temperament—disposition, attitude, habitual mode of approach to scientific problems—is that of the scientific psychologist. Whatever else Wundt might be doing, he also psychologised. He did not easily find himself; we have seen that there were years of wandering in the wilderness, and we have seen that the guidance which led him out of it is not readily determinable. When once he was free, however, he walked steadfastly in the path; year by year his psychology became sounder, as it also became more and more inclusive. A distinguished European psychologist wrote to me recently that he held no high opinion of Wundt's psychology because its theoretical views seemed to him to be nearly always wrong. Personally I do not greatly care about theoretical views; they are nothing more than an individual's blundering effort to bracket together and make manageable some large unruly body of observed facts. We may be sure, realising the limits of our acquaintance with fact, that whatever view we adopt will be inadequate, and we may fairly expect that increased knowledge will wholly discard it. We can only do our best with the facts available, as Wundt did, and trust to the future to do better by aid of further facts. But if a man is to gain his niche in history, he must have



the total vision, the generative idea. And for that reason I believe that when Wundt's special theories have utterly perished his fame will still endure; it will endure because, for all the hampering influence of the past, he established a new point of view and from it surveyed the whole scientific and philosophical domain. In this sense I am prepared to say that Wundt is the founder, not of experimental psychology alone, but of psychology.

#### PORTRAITS OF WUNDT

The earliest portrait of Wundt that I know of is the academic photograph, reproduced by G. S. Hall in "Founders of Modern Psychology," which shows him in three-quarter face at about the age of forty-five. The portrait is of especial interest because Wundt's right retina had not yet suffered the injury that led to strabismus. An academic photograph of some ten years later is an excellent profile picture. I have also a very good platinotype enlargement of a three-quarter face, made by the university photographer, C. Bellach, in 1897. The *Berliner Photographische Gesellschaft* publishes a reproduction of a painting (almost full-face) by Dora Arnd-Raschid, which is an admirable rendering of first impression and remains, to my mind, distinctly preferable to the later official photographs. N. Perscheid's photograph of 1904 (published by the *Berliner Photog. Ges.* and reproduced in the album of photographs edited by M. Brahn for the Leipzig University jubilee) has its merits of pose; but it, as well as the photograph accompanying Wundt's *Festrede* in the official Jubilee volume, gives him a look of stolidity which is altogether misleading. The current postcard photograph exaggerates this effect of stolidity. There are photographs extant of the group that gathered at Tambach in the Thüringer Wald on the occasion of Wundt's seventieth birthday; all four members of his family appear in them. The Jubilee book entitled "Die Universität Leipzig 1409-1909" contains a full-face pen-and-ink sketch by O. R. Bossert, which at first sight strikes one as caricature, but which takes on resemblance as one grows familiar with it. Wirth publishes as frontispiece to the 40th volume of the *Archiv* a pencil-sketch (profile) made shortly after Wundt's death by Felix Pfeifer.—These, aside from the print in the Open Court Series and a few unimportant reproductions in popular magazines, are all the portraits of Wundt known to me.

In 1905 (the year of the golden jubilee of Wundt's doctorate) a bronze plaque showing the face in profile was prepared by Pfeifer. The bronze is eminently satisfactory; the reproduction accompanying Wirth's memorial article is disappointing; the rounding of the temple and the hollowing of the lower cheek, both characteristic features, are largely lost. The bronze can be obtained in two sizes; Wirth gives price and other particulars. Some ten years later a larger bronze plaque was made by Max Lange. There is a reproduction in the *Leipziger Illustrierte Zeitung* (1916?). There is also a bust by Max Klinger. Wirth calls it "gewaltig, aber künstlerisch stilisiert." I have seen neither original nor any reproduction.—

I do not know if there are any official photographic memorials of the old laboratory in the *Konviktsgebäude*. I have a pencil-sketch, looking from the first room, with its stove and chronograph, through the *Vorzimmer* and past the resonance box for the giant fork to the entrance-door; and I have five amateur photographs of rooms, one of them showing Külpe lecturing in the auditorium. I shall be glad to know if there is anything else.

Wundt, in his historical article (*Festschrift . . . der Universität Leipzig*, iv., 1, 1909, 118 ff.), says that this old laboratory had five rooms; I imagine that one or two had been partitioned. At any rate I remember more. The *Vorzimmer* (1) was a narrow entry that served only as storeroom. Then came (2) the first room, with chronograph, case for tools and instruments, and table for optical work. Out of this opened (3) the dark room, in which "eine mit Rüböl gespeiste Moderateurlampe" used up more than its fair share of oxygen. Beyond the first room lay (4) the second room, with chronoscopes and instrument-case. Somewhere alongside of this, probably continuous with the dark room, was (5) Wundt's private room, which must have been served by a special staircase, since Wundt used to appear out of it and disappear into it without passing through other rooms. As I remember the glimpses through the open door, it contained nothing but a table and a couple of chairs. Finally, beyond the second room, came (6) the last of the suite, the *Lesezimmer*. Across the corridor were (7) a room containing the reaction keys and stimulators, electrically connected with (4), and (8) a small room containing the gravity phonometer, the Wundt pendulum, the Fechner pendulum, etc. If we count only (2), (4), (6), (7) and (8), we have Wundt's five rooms.

## THE CHILD MIND

By HENRY JONES MULFORD, M. D., Buffalo, N. Y.

The world always has found the mind of the child interesting. Heretofore that interest has been an idle interest aroused by seemingly incongruous manifestations within the child-mind; but latterly the interest has taken on a serious intent. The merely glancing curiosity has become a focussed curiosity. The incongruous has appeared so regularly and so constantly that there has seemed to be some order in its manifestations, and the world at last has been forced to change its attitude. The attitude now is scientific. The world now seeks for the reason behind the manifestations, and in that search is exploring the whole aspect of the human mind. But the quest has had its difficulties. This territory, a veritable *terra incognita*, has been difficult of approach. In such territory direction must be sought and paths cut; and it has followed, naturally, that the investigator has made many starts in false directions.

But now, while the quest is difficult, is it as difficult as it has been made? Is mind the unknowable thing it has seemed to be? It is if we accept mind as an entity by itself, as a superphysical manifestation; for then the method of its manifestation is difficult to understand. It is not, if we accept mind as a natural phenomenon, as a phenomenon having a physical basis; for then all that we have to do to make it clear is to discover its physical origin.

And here we do not have far to seek, not farther than the brain; for is not the brain the physical basis of mind? Is not the evidence all in favor of this hypothesis? The gradual development of the brain; the absence of mind where there is absence of brain; the imperfect mind where there is imperfect brain; the suppression of mind where there is pressure upon the cerebral cortex, and its return when the pressure is removed. We find in fact that, as is the cerebral cortex of man, so is his mind. Every cell in that cortex reflects mind, every swelling convolution is an index of its increasing power.

Mind, therefore, is a physical manifestation; and, as an individual expression, reflects the peculiarities of the individual through whom it is manifested. The manifestation

proceeds through the brain, but may be modified by the behavior of any of the other organs of the body. That is, the physical constitution determines the psychical constitution; the make-up of the individual determines his trend. That the mind is nothing beyond a physical expression must be so, for no living structure can express itself except in terms of its own cells: every tissue is limited by its own constitution. Among the living things upon this earth each separate group has its own radius of action; and in each group the individuals of that group have their own idiosyncrasies arising from individual peculiarities of structure. In man there is a greater individuality; there is a wider latitude in brain development; and it is this wider latitude that has brought about the development of the human mind. This greater radius of action has proceeded through the greater flexibility of the human brain. This brain has come into its own through the development of self-consciousness, through the faculty of conscious direction. *Brain has developed mind, and now mind is developing brain.*

A contradiction now becomes apparent. If the make-up of the individual determines the trend of his mind, how is it possible for that mind to choose its own direction? There may be discovered in this situation a suggestion of the highest importance leading to the proper understanding of the human mind; it contains a revelation of the process through which mind is developed. There is here a conflict; a conflict between the Past and the Present for the determination of the Future; the Past represented by the structure of the brain, the Present by environment as it acts upon that structure, and the Future by the result, that is, the individual. The conflict here is the old, never-ending conflict between heredity and environment as to which shall control the individual. And this is a real conflict. The Past, with its ancestral line extending back to the very beginning of human life, objects to resigning a control so long established; while the Present, *conscious of its own purpose*, demands that it be given a voice in the direction of affairs. The outcome of this conflict will be as the outcome of any conflict: the stronger will win. Following this rule the outcome will not be always to the advantage of the individual; he will not always have the choice as to his own direction. But there is a way to obviate this result. The individual may, if he wishes, make his own choice. But this will depend upon the state of development in which his mind rests, upon his degree of self-consciousness, *whether he knows what he is doing*. If he does know, if his state of conscious-



ness is such that his mind can rise above impulse, his future is safe in his own hands.

Finding, then, that mind is an expression of the brain, we find also that, following the law of development of animal organs, the expression varies with the age. In accordance with that law the child-mind is a primitive mind; it is the primitive expression of a primitive organ. In this organ the conflict between the past and the present is, though very acute, very one-sided. The past is the dominant influence, but the present is making strenuous efforts to secure a foothold. Heredity and environment are striving for the mastery, but heredity holds the advantage in that it is the older.

Our understanding of the child-mind, then, depends upon our understanding of the child-brain, how it is developed and what it means. The child-brain represents the primitive human brain, the brain of a million years ago. In this brain the conflict between the past and the present has only just begun. It is emerging from the purely animal stage, but it is carrying with it the attributes of that stage; the brute is becoming the man, but the brute brain, following the habit of another million years, still asserts itself. It is this double expression of function in the human brain that confuses us. The Past and the Present are two separate entities; but, using the same apparatus, they appear as one. Our confusion has arisen through our looking upon these two as being, both of them, manifestations of mind. And there has been the great error. Only one is a manifestation of mind, the other being merely reflex action. Mind is consciousness, is the knowledge how to direct the brain reaction; the reflex is pure automatism, the response to external stimuli without conscious direction. But the mind, while itself a directing power, may be directed by the reflex, even though the reflex lies outside of consciousness. This is, in truth, the dominant reaction within the child-mind. Let us see how this comes about.

Every individual living at this moment is the present manifestation of an ancestral line extending back into the very mists of the beginning. Every human being represents a line of human life at least one million years long, and of life behind the human of no one knows how many millions. It may be perceived from this how complicated a structure the human organism is. It is a structure built up by slow, toilsome effort through countless epochs, each epoch having left its mark thereon. That this is so is becoming more and more evident; the slowly accumulating evidence of man's past points always in the one direction. Is it not true that the living

things upon this earth to-day are but the descendants of other remote forms? Nature proceeds from one form to another; there is no spontaneous generation of higher forms. Man being a part of Nature can be no exception to its rules. He must have come from a lower form, and that lower form must have been an animal very closely related to him in structure. Of a truth the early man was little better than an animal. It must follow, then, that the man-brain in the beginning was no greater than its possessor, for the man and the brain are synchronous.

The transition from animal to man covered a period of great length. And this was in accordance with another law of Nature: the longer the period of development, the better the individual. In the beginning Nature made it easy for the developing man, for it was her purpose to nurse him into a strength that should endure through the long ages that were to be his upon this earth. The primitive environment was a supremely comfortable one; it was, in reality, a lazy one, for it was non-stimulating. The climate always was mild, food always was within easy reach; the primitive mind had little to disturb it. Life was at ease. The primitive mind did not even have to think for itself, its environment not being thought-inducive; and, not being obliged to think, the primitive mind remained as it had begun, a merely automatic function. It responded to external stimuli through the reflex; what little thought it had was reflex thought; mind was not yet strong enough to control the reaction.

The animal, whether brute-animal or man-animal, is a creature of reflexes: he is governed by reflex action. In the man-animal these reflexes manifest themselves in three directions. There is the basic reflex or cell irritability, there is the motor reflex, and there is the thought reflex. If we examine these carefully we shall find that they have developed as the human organism has developed; we find, in fact, that the organism has developed through these. If we go back a hundred million years to the solitary cell, to the time when it is estimated that life began upon this planet, we catch our first glimpse of the reflex. There we find cell irritability answering every purpose of the cell. The reaction there is relatively simple, being the reaction to a primitive environment. But now, advancing our investigation a few millions of years, we come to a period where the single cells have associated together to form organs, and the organs to form organisms. In these positions independent cell-action would mean inharmonious action; for not only are the cells grouped together,



but they also have become differentiated. Cells thrown together into a group cannot act as independent cells, they must act together; and the different groups also must act together, else the organism will disrupt itself through the clashing of independent movements. And so we find here the demand for something that will harmonise the activities of the various independences. The primitive nervous system answered that demand, a system made up of a few nerve fibres conducting impulses to a central station, a station that was nothing more than a receiving station. Again jumping our investigation ahead, we find ourselves at a period when the organism, having reached to a very high degree of development, needs self-direction in order to utilize its higher power. It is at this period that we find the human brain coming into existence.

All of these reflexes interest us, each one according to its position. We are interested in the first, in cell irritability, because it is the starting point of all the reflexes. In the primitive cell the reflex was the expression of the reaction between the cell protoplasm and its environment. The purpose of this reaction was to maintain the well-being of the cell. It was, primarily, a reaction to food, but out of it have been developed consciousness and sex. In its search for food the cell came into contact with three varieties of substance: substances that maintained the integrity of its protoplasm, food; substances that were harmful to its protoplasm, foreign bodies; and the substance of other cells that revived the cellular protoplasm and furthered the increase in the number of cells through segmentation. Out of contact with foreign bodies was developed tactile sensation, and, out of tactile sensation has come consciousness; out of intercellular contact has come sex. Consciousness, being a supremely higher expression of function and far more subtle, has required the full term of life upon this planet for its development; while sex, being an earlier requirement of animate organisms, was developed very early in the evolutionary process. It might be said, too, that out of this reaction to environment comes pathology. Overstimulation of the cell protoplasm produces excitement, and overexcitement in its turn produces fatigue, and there will follow either an impairment of function or its entire suppression.

Coming now to the motor reflex, we reach the reflex that is of the first importance in our present discussion, for it is through this that the mind had, *and has*, its real beginning. It was the first in actual brain development; it was the first visible reaction of the primitive nervous system; and it has

been the dominating influence in the development of that system. Development of the higher centres adjacent to the motor has followed through the motor. The primitive central nervous system was, as we have seen, nothing more than an automatic station where the incoming stimuli were received, synchronized, and returned as motor impulses. But these reactions were inflexible; they always were the same. A given reflex always performed in the same direction. Being automatic it could not vary; its mechanism was set in the one certain direction. And, later, when the real brain came into action, this method persisted; for this brain having developed through the primitive system could act only after the manner of that system. Even though the animal possessed a brain his actions were not man-actions, for he did not yet possess man-consciousness; his brain was not yet fully organized. It was not yet able to direct itself. At the same time its reach was beyond that of the primitive station. It was the function of the primitive station to preside over the non-conscious activities of the organism, while the brain, ultimately, was to preside over the conscious; the one synchronized the organic activities, while the other was to direct the organism as a whole. The one must, of necessity, be automatic; and the other, while destined to proceed beyond mere automatism, had to begin as had the other. And so, when we come to the primitive man-animal, we find his actions the actions of the primitive brute-animal. They did not proceed through the thought reflexes of the man, they came through the motor reflexes of the animal. His radius of action, therefore, was limited; being reflex it was inflexible.

A third and last jump along the evolutionary path advances us to the period of thought-development. The primary influence in the production of thought was environment. Brain already had been developed, but it was brain that was not conscious of itself. But now great variations took place in environment. Extreme mildness of atmosphere gave place to extreme cold, and cold again to mildness; fearful convulsions of Nature altered the face of the earth; food became more and more scarce, and other living things needing food more and more numerous. The most important event in the history of primitive man took place during this period: the differentiation of his hands. While yet an animal he had learned to use his fore-feet as hands; but this use was automatic in that he used both hands as one. But now a varying environment brought varying uses for his hands. The work of the two hands was becoming finer, that of the right pre-

ceding and going beyond that of the left. It was the diverse influences of a varying environment that developed conscious man out of a merely automatic animal; but it was the increasing importance of his upper extremities that furthered the development of man's consciousness.

The new man was making new movements with his hands, movements that were, literally, out of reach of the brute; and he was associating those new movements with his higher reflexes. *His consciousness was taking hold of them.* The new movements meant different and finer adjustments of his muscles, meant independent action of the various groups, and a finer co-ordination between the groups themselves. They meant, also, co-ordination between the reflexes of those muscles and the higher reflexes; they meant that thought was becoming associated with action. And this increase in the reflex radius made further and further demands upon the brain. Following these demands the brain was forced to adapt itself to the new stimuli coming in to it.

It is to be regretted that we have no fossil remains of the brains of our remote ancestors, that we might compare the structure of the perhistoric brain with that of the brain of to-day. A number of skeletal remains have been uncovered in the deep layers of the earth's crust, but the soft parts, because of their very nature, were not able to endure with them. But advancing knowledge, knowledge built upon research and reason, is bringing to us a revelation of what the primitive brain actually was. In the light of this knowledge we are catching glimpses of the brain of primitive man and of that of his immediate ancestor, pre-man. This knowledge comes to us from two directions: from the examination of skulls found among the skeletal remains of primitive individuals; and from the examination of living brains that seem to approximate, in size and structure, those of the primitive animal.

Inspection of a primitive skull reveals the following facts in regard to the individual to whom it belonged. First, the layer of the earth's crust in which it is found will determine the epoch of the earth's history to which the individual belonged when living. Secondly, the external configuration of the skull will determine whether the individual was brute or man. The determining factors here are: size and shape of the skull as a whole; size and external configuration of the individual bones, this applying especially to the bones to which the muscles of mastication and to those to which the muscles of the neck are attached. Thirdly, the internal configuration

of the bones of the skull will give an idea of the development of the brain that occupied the skull cavity; and the size of the cavity will give the size of the brain.

The living brains of to-day that will help us to an understanding of the primitive brain are the brains of those primates known to us as the anthropoid apes. In very fact, if the brains of the entire monkey series be examined, a very suggestive progressive relationship in regard to size and function will be uncovered. Let me emphasize this by going over, in a very superficial way, several of the varieties of brains belonging to the monkey family, limiting the examination to the lateral aspect of the cerebral hemispheres.

Beginning with the brain of the *marmoset* we find the outer aspect of the cerebrum completely smooth, there being only the Sylvian fissure in evidence.

In the brain of the *capuchin monkey* the convolutions begin to appear, the smooth surface of the cerebrum being divided into broad convolutions by fissures more or less superficial. Of this surface it is seen that the sensori-motor area, the area embracing the precentral and the postcentral convolutions, occupies about one-third, and the areas of the frontal, the temporal, the occipital, and the parietal lobes each about one-sixth.

The brain of the *bonnet monkey* resembles very closely the brain of the capuchin monkey. There is the same general configuration, with the same relative proportion of cortex in the different areas; but here there is a greater attempt at fissure production, and hence a greater area of cerebral cortex.

In the brain of the *yellow baboon* we have an organ larger than either of the preceding, but one in which the configuration of the cerebrum is much the same. But here the fissures have become decidedly deeper and more numerous, with a consequent greater number and a finer arrangement of the convolutions. In this brain the sensori-motor area occupies about one-fourth of the cerebral surface, and the other areas about the same relative proportion of surface. A point to be noted here is that, while the relative proportion of the frontal lobe is the same, the actual area of that lobe is greater in this brain than it is in the brain of the capuchin monkey.

Coming now to the smallest of the anthropoids, the *gibbon*, we enter a region in which the brain topography suggests something more than mere brain. The sensori-motor region is still prominent; but those areas closely adjacent to it, the frontal and the parietal, the so-called areas of the higher centres in man, have altered. In the frontal and the temporal



the fissures have become deeper and, for the first time, three distinct convolutions have appeared; in the parietal the convolutions have become more complicated.

The brain of the *orang*, one of the larger anthropoids, presents a still more complicated surface, especially in the frontal region. The convolutions of the entire surface of the cerebrum are larger and extend more deeply than the convolutions of the cerebra of the previous brains.

The last anthropoid brain which we shall discuss here, that of the *chimpanzee*, is the most interesting. In this brain there is a very close approach to the human type. Its fissures are deep and of good length, especially the Sylvian and the central; its convolutions are well modeled and of a more complicated pattern. Here for the first time we find the frontal region exceeding the sensori-motor in surface. The third frontal convolution has become tortuous and bent upon itself, being suggestively like the same convolution in the human brain. The parietal lobe has widened, and the temporal has increased its convolutions.

Comparing, now, the human brain of to-day with the preceding series we note the ascending similarity; and the thought comes to us that in evolutionary history these brains must all be chapters in the same story. We note the general increase in cortex over the entire cerebrum, but more specially in certain regions. The frontal, parietal and temporal lobes have now become well-defined portions of the brain. The difference here is the difference that accords with the new function taken on by the human brain, that of mind.

I am making no definite assertions as to the position of the immediate ancestors of man; I am saying, merely, that study of the brains of the monkey family will suggest much in regard to the developmental plan of the human brain. Interest in this study will direct our attention in three directions: first, to the external configuration of the brain; secondly, to the microscopic structure of its convolutions; and thirdly, to the configuration of the inner table of the skull against which the convolutions lie.

Following the suggestions it will not be difficult to trace the growth of the man-brain out of that of pre-man.

The pre-brain was a negative brain. It was not self-assertive, self-directing, self-conscious; it was entirely vegetative, merely a reflex station. It was not active, it was reactive; it was not conscious, it was non-conscious. Knowing how this brain performed, we can gather some idea as to its form and structure. This brain, having been a reflex brain, had

as its main centres those that were purely reflex, the motor centres and those having to do with the special senses. Among these the area comprising the so-called sensori-motor region, the precentral and the postcentral convolutions, was the oldest. It may be that the postcentral convolution is older than the precentral. The fact that the sensory fibres are medullated before the motor would indicate that; but this is not a vital point. The two work so in unison that they may be considered to be of the same age. Following these in regular developmental sequence came the centres for smell and taste in the lobus pyramidalis, the centre for sight in the occipital lobe, and the centre for hearing in the temporal.

These regions were the oldest regions of the animal brain; their centres were the first active centres, and the limit of action, of the pre-brain. If it were possible to inspect a pre-brain, and its accompanying skull, we should find that the brain was small and symmetrical, with the convolutions carrying the above-mentioned centres the most prominent ones of the entire organ. There would be other cortex, but there would be only enough to allow for the natural expansion that must follow in an organ of this character. We should find some cortex anterior to the precentral convolution, a very rudimentary frontal lobe; we should find some between the postcentral convolution and the centre for sight, a beginning parietal lobe; we should find a very small amount below the first temporal convolution; and there would be a small silent area in the occipital lobe. Inspection of the inner table of the skull enclosing this brain would reveal the impressions made thereon by the convolutions containing the primitive centres, while examination of the centres themselves by means of the microscope would show the characteristic cell and fibre arrangement of to-day in a primitive but well-marked stage of development. In the excess regions the microscope would reveal only a few scattered very rudimentary cells and fibres.

Advancing a further stage in the animal phylogeny, to a period just preceding the emergence of the man-brain, we should probably find a brain of the type of the chimpanzee-brain of to-day. In this brain we find the primitive centres still prominent, still making the deeper markings upon the inner table of the skull; but we find, also, that these markings have altered. The positions of the deeper markings have changed: they are more extensive and are further apart; and other markings are beginning to appear. The areas of the primitive centres have increased somewhat, while the increase in the new areas has been more marked. The region



anterior to the precentral convolution has grown forward into a distinct lobe, with deep fissures and complicated convolutions. The third frontal convolution, *Broca's convolution in the human brain*, has become more complicated than any of the others of this region, while those convolutions closely anterior to the precentral are the next in the order of development. In the parietal and the temporal regions corresponding increases in the convoluted surfaces will be found.

We see in this arrangement of the cortex of the animal-brain what seems to be preparation for the higher function of the man-brain; in thus expanding, the animal-brain is laying the foundations of those centres that, arising through its own primitive centres, are to raise the animal to the man class. Comparison of the microscopic structure of the chimpanzee-brain with that of the man-brain will demonstrate this.<sup>1</sup> The minute structure of the cerebral centres of the chimpanzee will be found almost to duplicate those of man. The difference is the difference that might be expected between an undeveloped and a developed brain.

Another step with our phylogenic seven-league boots and we find ourselves actually within the domain of man. The brain of man, well-advanced along the developmental path, is before us. Looking at it carefully we note the difference between this brain and the brain of pre-man. The pre-brain is small, symmetrical; the man-brain is large, asymmetrical. The difference is eminently one of development; development has increased the scope and the complication of brain-function, and has caused the asymmetry. The newer regions have doubled their capacities in the man-brain; but in the development of the new centres the development has proceeded unevenly, some have developed earlier, and some faster than others; each centre, produced through long ages of effort, has its own position in a regular developmental sequence. It is thus that the human cerebra have developed irregular and unequal contours.

In this brain the promise of the pre-brain is fulfilled: the man-brain has become an assured fact. The centres most concerned in this advancement are those situated within the frontal lobe: the centre for speech, and those centres which are the outcome of the differentiation of the hands. The parietal, too, has become of great importance, but its importance is secondary to that of the frontal. The frontal leads in those functions which are peculiarly man-functions. The new func-

<sup>1</sup> See "The Localization of Cerebral Function," by Alfred W. Campbell, for very exhaustive researches in this direction.

tions are associated functions; that is, they are associated with other functions in their operating mechanism. This association is two-fold: with centres immediately adjacent, and with centres in other regions of the brain. As an illustration of the first we note the centre for speech in the frontal lobe, which seems to be merely an enlargement of the motor area; the centre for word-hearing in the superior temporal convolution, which is actually an extension of the centre for hearing in the same convolution; and the centre for word-seeing in the gyrus angularis, an offshoot from the centre for sight in the adjacent occipital lobe. As an illustration of the second we note the association of the centres in the precentral and in the frontal regions to the visual centre at the posterior extremity of the cerebrum.

But these new centres, while offshoots of old, have not been made in a moment; in the upbuilding of brain-tissue Nature requires time. There is no spontaneous creation of tissue here, it is the tedious process of slow ages. And in this tissue there is a peculiar situation. While it gives no evidence of activity during its upbuilding, still it is not inactive; while it cannot respond to external stimuli, it can receive them. This is in line with the developmental plan. Sensory fibres are medullated before the motor; sensory stimuli are received long before motor responses can be returned. It is the prickings of the ingoing stimuli that develop consciousness in the centre. This is not to say, however, that there is no attempt at expressing itself on the part of the centre during its upbuilding. It is possible for this centre to make the attempt, but the action resulting from such attempts must, of necessity, be imperfect. No centre is capable of normal action until the structural elements of that centre have become fully developed.

This is shown in the development of the speech centre. Pre-man did not have speech, but he did have sound, uncouth, unmodulated noises. In making these noises he used the muscles concerned in the act as he used his other muscles, as group muscles; upon the impulse *all* the muscles acted together. The action was entirely motor, merely reflex action; there was no consciousness behind the act. After a time the animal found that he could control his voice somewhat, that he could change from one tone to another, that he could modify the uncouth noises proceeding from his throat. He still made the uncouth noises under the stress of sudden emotion, but at other times he was able to guide his utterances into a kind of chatter. At first this was just an aim-

less chatter, but eventually he was able to inject a meaning into the sounds issuing from his larynx. He had become conscious of his larynx, and that consciousness gave to him the power to direct its action. But that power was a long time coming to him; from uncouth noise to co-ordinated sound covered a vast period of developmental effort. The mechanism of speech was too complicated to be adjusted by any shorter process.

This is the method through which the speech centre was developed, and it is the method through which all the brain-centres have been developed; it is the method through which the man-brain has been developed out of the animal-brain. It also is the method by which the individual brain of to-day is brought into function; that is, it is the method through which the adult brain is developed out of the child-brain. The child-brain is not born already developed, *it is born to be developed*. Its position is that of the man-brain during the childhood of the race: it is on the threshold of a higher life. But while its position is the same, its condition is different. The brain of the primitive child was an animal-brain, pure and simple. Its active centres were only those in the primitive areas; the other centres were not active for the reason that they were not yet fully developed, they were in the process of being laid down. *These centres were being created*. In the child-brain of to-day the condition differs in this respect: while the child-brain is an animal-brain in one direction, it is a man-brain in another. It is an animal-brain in respect that it is active mainly through the primitive centres; and it is a man-brain in respect that the man-centres already are laid down. It is actually an embryonic man-brain, for the foundations of the man-brain are there; the foundations are not being laid down, they are finished.

While, however, the child-brain is born in this condition, it is not yet a completed brain. As has been said, the child-brain is a brain to be developed. It is ready for action, but as yet it cannot act, that is, as a man-brain. The new centres, having been created, now must be brought into efficient function. And so, while the child-brain is getting its full growth, these centres are busy receiving stimuli and training themselves for action. At the end of eight years, post-natal, the human child-brain should be able to take care of itself.

During the period of life of the human brain beginning at the fourth month of pre-natal life and ending at eight years post-natal, it exhibits all the phases of development that we imagine the racial brain to have experienced. Up to the period

of the fifth fetal month the cerebral surfaces are completely smooth, the Sylvian fissures being the only fissures in evidence. During the fifth month the other fissures begin to appear, the calcarine, hippocampal and collateral on the median surface, and the central, precentral and the superior temporal on the outer aspect, being among the first. At seven months the surface of the cerebrum is well convoluted, while at nine months the outlines of all the convolutions are completed. Up to this point the size and contour of the human brain remain small and regular, resembling very closely the outlines of the anthropoid brain, but differing from it in one important respect: the extent of the frontal region. After birth the human brain continues its growth; this further growth placing it well in advance of the primitive brain it was. In short, the period of intra-uterine life might be likened to the evolutionary period during which the foundations of the man-brain were laid down, and the period after birth to the period during which the man-brain enlarged those foundations.

Following our discussion of the phylogeny of the human brain we now are the better able to understand the meaning and the method of the child-mind. It now becomes certain that the child-mind is but the expression of a developing brain, and that the expression follows the method of the organ from which it emanates. A further fact here, the puzzling factor in the reaction, is that this expression is the expression of a new function in the course of its development out of an older, firmly established function, the development of the man-function out of the animal-function. This new function varies as it grows, and as it grows has to fight its way against the dominance of the old function. The man is in a contest with the animal: the Present is in a contest with the Past. Our position here, then, should be that of supervisor, of director. We shall need to assist the new function to establish itself, we shall need to assist the man in his fight against the animal, we shall need to assist the Present in its contest with the Past. Left to itself the new might, out of sheer inertia, allow the old to overbear it.

If the child-mind is the equal of the child-brain, then the child-mind is a primitive mind, just as the child-brain is a primitive brain, and the method of the child-mind will be the method of the primitive brain. We saw that the centres of this brain came into action one after the other in an orderly sequence; and we saw further that these centres were developed through the reflex, that their reactions were touch-and-go reactions. The action within the child-mind is purely reflex;



it responds to environment automatically. The mind comes into development slowly, in response to environmental stimuli repeated over a long period. *The centres of the brain have to be prepared for function before they properly can functionate.* To be sure, the foundations of the centres are there, but they are mere foundations put there for the support of the superstructure. But we must not lose sight of one very important fact here: the foundation is the foundation of the man-brain. Therefore, while the basic reaction of the child-brain is reflex, following its animal origin, the secondary reaction is that of the man-brain. In the child-brain these two are very closely associated, the first beginning during the intra-uterine period and the second manifesting itself early in the post-uterine.

The growth of all the human brain-centres illustrates this; but, for a specific illustration, let us touch again upon the centre for speech. We have noted the disposition of Broca's convolution in the anthropoid brain and in the man-brain; and we have noted, also, how function seems to follow that disposition. We note the progression from no-speech to full speech; and, following the development of the brain of the human infant, we find this same developmental sequence there. From the moment of his birth the infant begins to make sounds through his larynx; but these first sounds are not speech-sounds. They are mere noises, uncouth, incoordinated cries. They are signals of distress, that is, of pain, of hunger or of shock. They are pure reflex manifestations; and the capacity for these manifestations never is lost. But after a bit we find that the baby has increased his vocabulary, as it were. He begins to laugh and then to coo; and then, as his development continues, as he gets older, his utterances take on a letter or a syllabic form, and later still a word form. Following the coo the effort is over a single syllable, such as "da," for instance. But the baby will not be able to say "da" at first. He will say "d," haltingly, in the beginning; and then, as the sound comes more and more easily, he will repeat the letter over and over in quick succession. That established, it will be an easy step to the full syllable, and he will use the "da" as he did the "d." Frequent repetition of "da" automatically creates a word, and soon we hear the baby saying "da-da." Then he learns to associate that word with his father, and speech for him has been established. The centre controlling speech has come into consciousness; and from "da-da" to "ma-ma" and other short, intimate words no great effort is required.

Two factors are behind the progress of the infant brain: that it is an embryonic man-brain, and that the baby is in close association with his environment, represented here mainly by his mother. It is the example of the mother that has encouraged the baby-brain to exert itself. She has laughed and cooed with the baby, and has urged speech upon him; but all her urging never would have made the baby-brain exert itself if that brain did not have the power to exert itself. If it were not an embryonic man-brain it could not respond to the man-stimulus. The human child takes in words, recognizes them and then repeats them; but he does these things because his brain belongs to the man-class, because his brain is developed for the purpose. The man-function responds to the call of the man-environment, but the response in the beginning is only a reflex response; the reaction is the primitive reaction, for the mechanism is still primitive. But soon the constant effort at responding to the incoming stimuli enlarges the grasp of the cerebral centres, they become more and more conscious of what they are doing, and they begin to lose that purely animal characteristic, the reflex. But in every stage of the development of the child mind the reflex remains the dominant factor. Consciousness should be the dominant power in this brain, but it is not. Consciousness does have the directing power, but it does not have the power actually to direct. That is, consciousness is not yet strong enough for independent action; and, as with consciousness comes thought, then thought can have no greater strength than consciousness. Thought itself is, at this stage, little better than reflex action. The fact of the dominance of the reflex is the fact of greatest importance in brain development. The reflex dominated the primitive brain, and the reflex dominates the child brain. But that is not the reaction most to be desired in the human brain. It is the thought reaction that should have first place, *directed action*; consciousness should have supreme control.

We have, then, in the mind of the child factors that are subtle and far-reaching. We have the Past, an influence-complex that reaches up a thousand hands out of a loosely knit and interminable ancestral line; we have the Present, an influence-complex developed out of the action of environment upon the millions of cells that make up the cerebral cortex. It is the reactions between these complexes that determine the condition of the child-mind, or of any mind. It is the Present-Past reaction that gives the interest to the subject: it makes the child-mind, in very fact, the most in-



teresting thing in this world. The problem to be worked out is a problem in development: how may the child-mind be developed to its own best advantage? As we have seen, this is a matter of brain-development, not to be understood until the whole process of brain-development is understood. The basic facts here are these. The development of the man-brain out of the animal-brain, and hence the development of the centres of the man-brain out of the centres of the animal-brain; the development of the sensory fibres before the development of the motor; and the dominance of the reflex.

The development of the child-mind is merely the development of consciousness in the child-brain; the development of a man-power brain. But the method of developing that consciousness must be the method followed by Nature. It must follow through the reflex, and is entirely a matter of training, a training that is directed through the motor centres. Each centre must be approached in the direction of its origin. We must remember that each centre has a regular developmental position and its fibres a developmental sequence; first the afferent, next the efferent, and then the association. The afferent stimuli are the ones that arouse a centre to action and that give its reactions smoothness. After a centre has undergone this training for a certain length of time it becomes able to control its own machinery; the centre then has developed its own consciousness. But that is not enough, so far as the mind is concerned. It would suffice in the case of a purely automatic brain; but it does not suffice for independent mind. Making each centre independent makes for disharmony. Centres working alone do not work together. The ultimate endeavor, then, in striving to develop the child-mind, is, while we are developing consciousness in the centres, to make that consciousness overlap from one centre to another, to bring about an interaction between the centres. The aim is, through the development of supreme consciousness, to convert the human brain into a symmetrical and a harmonious organ; that is, an organ fully developed and fully able to take care of itself.

# ON THE RELEVANCY OF IMAGERY TO THE PROCESSES OF THOUGHT<sup>1</sup>

By CLAIKE COMSTOCK

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In an article in the *Zeitschrift für Psychologie*<sup>2</sup> Dr. Koffka of Giessen says in criticism of an analysis of "Conscious Attitudes":<sup>3</sup> "It is obvious that analysis meant for the author and her observers nothing else than the exhibition of the sensory contents present at any given moment. . . . These sensory contents may [however] be irrelevant to the thought, or may be the necessary condition of the arousal of the thought, or may finally be the thought itself." Clearly the value of the analysis of a thought-process is dependent upon the relevancy of the contents which constitute that analysis to the thought-process analyzed. To find a criterion of relevancy and irrelevancy we undertook the following series of experiments. More particularly, we hoped to secure a basis for answering such a question as that asked by Koffka: How do we know that any sensory content is relevant or irrelevant to a thought? We have concerned ourselves especially with the relation of imaginal contents to thought, since it seemed wise to limit the problem. The *O's* have, however,

<sup>1</sup> From the Psychological Laboratory of Cornell University.

<sup>2</sup> 63, 1912, 219.

<sup>3</sup> H. M. Clarke, *Conscious Attitudes*, *Am. Jour. Psych.*, 22, 1911, 214-219.

frequently mentioned in their reports the parts played by sensory kinaesthesia and by feeling.

Relevancy may be of two kinds, material and logical. It is possible for imaginal contents to be materially relevant but logically irrelevant to a thought. For example, in reply to a question concerning the number of small boxes contained within larger ones, an *O* gave the correct answer, 21. When asked to report the imagery upon which this answer was based, he described a complex visual image of white boxes about six inches square with a grey interior. Logically, the grey and the white and the dimensions have nothing to do with the solution of the problem. The boxes might just as well have been red, or have had no color ascribed to them. Materially, however, the imagery was relevant, since for this particular *O* the meaning 'box' was carried by the particular kind of box described.<sup>4</sup>

We started out with the belief that we might find in the analysis of thought-processes a good deal of irrelevant material. We expected to have reported contents irrelevant to the thought concerned. Our task was then to be a determination of the psychological criteria of this irrelevancy. The results of our experiments have, however, forced us to the opposite point of view. They show that, if imagery is present as part of the contents of thought, it is *ipso facto* relevant to the thought. This conclusion we reached only at the end of a series of experiments, in every one of which we had been 'set' to find irrelevant imagery in Koffka's sense.

We attacked the problem first on the side of relevancy, though always with the expectation of getting indirectly at irrelevancy. Our task was in part one of method, and the attempt to secure certain experimental conditions explains the sequence of the separate experiments which we undertook.

#### EXPERIMENT A. 1. PROBLEMS

Our aim here was to study the imaginal contents of thought with reference to its uses and relevancy to the thought. The method consisted of presenting to the *O* a simple problem to which he was asked to give an answer. After the answer had been given, he was asked to report the experiences upon which it was based. There were 83 problems or questions which may be roughly classified as follows:

##### 1. Arithmetic problems (9).<sup>5</sup>

<sup>4</sup> Cf. H. L. Hollingworth, *The Vicarious Functioning of Irrelevant Imagery*, *Jour. Philos. Psych. Sci. Meth.*, 8, 1911, 690.

<sup>5</sup> The numbers in parentheses refer to the number of problems of the various kinds.

Ex. At ten cents a yard, how much will eighteen feet of cloth cost?

2. Ingenuity problems (12).

(a) Easy (9).

Ex. A man wanted to catch a kitten, but the kitten ran up a tall tree which no person could climb. How could he get the kitten without hurting it?

(b) Difficult (3).

Ex. Out of 6 toothpicks make 4 equilateral triangles each one of whose sides shall be as long as a toothpick.

3. Abstract problems (3).

Ex. If the possession of money or wealth in any form should come to be regarded as dishonorable, what significant changes would result?

4. General Information problems (24).

Ex. Where is the painting, Mona Lisa?

5. Completion problems (4).

Ex. Supply the missing letters: F-r o-f-c-a- b-s-n-s- o-l-.

6. Enumeration problems (8).

Ex. If a box has 4 smaller boxes inside of it, and each one of the smaller boxes contains 4 little tiny boxes, how many boxes are there altogether, counting the big one?

7. Action problems (6).

Ex. Suppose that you stooped down to lift up a large bucket full of water, but that as you stooped down to lift it up it proved to be empty. What would happen?

8. Simple Judgment problems (5).

Ex. What is the thing to do if you go to sleep on the train, and do not wake up until you are several miles past the station where you wanted to get off?

9. Comparison problems (6).

Ex. If grey is darker than white, and black is darker than grey, what shade of those named in this sentence is lighter than grey?

10. Direction problems (3).

Ex. Suppose that you are going north, then you turn to your left, and then to your right. In what direction are you going now?

11. Imaginal problems (3).

Ex. Suppose that it is fourteen minutes before three o'clock. Now suppose that the two hands of the clock were to change places, so that the large hand takes the place of the small hand and the small hand takes the place of the large hand. What time would it then be?

We tried to include problems of various types, ranging from very simple questions, to which the answers were given immediately and automatically, to more difficult problems where complex processes of thought were involved. We hoped also to give opportunity for the use of different kinds of imagery, as visual (group 11), kinaesthetic (group 7), verbal-motor (group 1), etc; though we realized, of course, that the type of imagery used depends principally upon the imaginal type of the O.

At first the problems were typewritten and given to the O's to read. With this procedure it was, however, impossible to control conditions, since the O's tended to glance back over the problem, even though only one reading was formally allowed. This source of error prevented the taking of a time-record, which is sufficiently rough at best,

since here the reading of the problem and its solution were inextricably combined. We then changed our procedure; *E* read the problem to *O* who was seated with his eyes closed and his back to *E*. When the last word of the problem had been read, *E* started the stop-watch, and stopped it when *O* gave his answer.

The instructions were: "I shall set you a simple problem. Your immediate task is to solve the problem. After you have reported your answer, I shall ask you to describe as well as you can the experiences upon which your answer was based." Later, to provide for a report in attributive terms, we changed to the following instructions: "I shall ask you a series of questions. Please give your answer as soon as it is ready. After you have answered, report so far as possible in attributive terms the experiences upon which the answer was based."

There were five *O*'s,<sup>6</sup> all of whom were graduate students or instructors in psychology; Miss A. H. Sullivan (*S*), and Messrs. H. Sheppard (*Sh*), M. J. Zigler (*Z*), H. S. Liddell (*L*), and L. B. Hoisington (*H*). Four of these *O*'s, *S*, *Sh*, *Z* and *L*, worked two periods a week; and one, *H*, worked one period. The observation-periods were usually one hour.

An analysis of the reports shows that imagery was used in a number of ways in the solution of the problems. A table indicating the different uses and the number of instances follows.

Uses		Number	
1. Anchor			
(a) Illustrative	136		
1. Memory experience	5		
(b) Non-illustrative	31		
(c) Combination of a and b	10	182	
2. Subject-matter of Problem			
(a) Changing	155		
(b) Fixed	57		
(c) Combination of a and b	9	221	
3. Regulation of Problem			
(a) Formulation	20		
(b) Anticipation	2		
(c) Criticism	17	39	
4. Means of Escape from Problem	5	5	
5. Illustration of the Answer	16		
(a) Reinforcement	5	21	

Total number of reports = 308.

In 21 instances the answer came as a sensorimotor response.

<sup>6</sup> Primarily the 'observers' in this study were 'subjects' who were performing a set task. We have named them 'observers' simply because our aim was to get them to describe the processes correlated with the meaningful stages in the performance of a task.

Illustrations of the different uses taken from the reports follow.

### 1. *Anchor. (a) Illustrative*

Problem 27. If the conductor on an Ithaca street-car rings up in one trip 41 fares, how much money has he taken in?

Report: "As the problem was read through, there was a scrappy visual image of something that meant 'conductor' (upper part of him) and of the tally machine at the other end of the car" (H).<sup>7</sup>

#### 1. *Memory*

Problem 22. What would you do if a person who you know is crazy calls you ugly names?

Report: "Visual image of a man sitting on a rock. It carries the meaning of the time when I was very small and one of our neighbors went crazy and my father struggled to keep him quiet" (Sh).

#### (b) *Non-illustrative*

Problem 56. If James had four times as much money as George, he would have sixteen dollars. How much money has George?

Report: "The two names were held by visual-verbal imagery. It meant the names 'James' and 'George' written in white on a black surface. These fluctuated in clearness. Occasionally both were present at the same time. They were spatially separated" (Z).

#### (c) *Combination of (a) and (b)*

Problem 64. What makes salt cake?

Report: "A visual image of a little pile of white salt. Verbal-motor repetition of problem: 'What makes salt cake?'" (Z).

### 2. *Subject-matter of problem. (a) Changing*

Problem 75. From what other method of transportation are the terms used on railroads taken?

Report: "Visual image of a blue-brownish mass. Meant 'boat'. Vanished quickly. Then a visual image of a coach going along a dusty road. In verbal-motor imagery the words: 'carriage,' 'coach,' 'pull-man'" (L).<sup>8</sup>

#### (b) *Fixed*

Problem 25. If the two diagonals of a square are drawn, how many triangles are thus formed?

Report: "Visual image of a square with the diagonals not completely filled in. The field is white and the lines black. I saw part of all four triangles" (L).

#### (c) *Combination of (a) and (b)*

Problem 67. Name three countries of Central America.

Report: "A visual image of a map of South America with its countries. Verbal-motor imagery of naming the countries (from the map) before I spoke them" (Sh).

<sup>7</sup> Cf. K. Bühler, *Über Gedanken*, *Arch. f. d. ges. Psych.*, 9, 1907, 353; H. J. Watt, *Experimentelle Beiträge zu einer Theorie des Denkens*, *ibid.*, 4, 1905, 361 ff.; and A. Messer, *Experimentell-psychologische Untersuchungen über das Denken*, *ibid.*, 8, 1906, 67 ff. The reports cited furnish instances of our 'anchoring' imagery.

<sup>8</sup> For illustrations of changing imagery cf. A. Messer, *op. cit.*, 57.



## RELEVANCY OF IMAGERY TO PROCESSES OF THOUGHT 201

### 3. Regulation of problem. (a) Formulation

Problem 10. A boy was sent to the river to bring back exactly 7 pints of water. He had a 4 pint vessel and a 9 pint vessel. Show how he can measure out exactly 7 pints of water, using nothing but these two vessels and not guessing at the amount.

Report: "A visual image of two vessels, glass. One was half of the size of the other. One meant '4' and the other '9.' Then verbal-motor imagery meaning: 'You'll have to solve by interchanging in two vessels'" (Z).

### (b) Anticipation

Problem 68. You say a flock of sheep, but a what of mackerel?

Report: "A visual image of sheep. Then I anticipated what was coming. This was carried by visual imagery of mother's flock of white leghorns and verbal-motor imagery in the naming of them" (Z).

### (c) Criticism

Problem 17. What holiday comes nearest the middle of the year?

Report: "I thought of Christmas. This was carried by verbal-motor imagery" (L).

### 4. Means of Escape from the problem

Problem 70. What is a firkin?

Report: "I don't know. I thought of Oscar Firkins, a professor of English. This was carried by kinaesthetic and verbal-motor imagery" (S).

### 5. Illustration of the answer (coming after the answer)

Problem 47. Suppose that you are going upstairs in the dark and think that there is another step ahead of you. If there isn't, what happens?

Report: "Following the answer I had a visual image of the head at the stairs, and of a person there with his head down" (H).

### (a) Reinforcement

Problem 6. Which is heavier, a pound of lead or a pound of feathers?

Report: "The problem was solved at the end of the reading. I had a visual image of the word 'Neither' and a period after it. The capital 'N' was typewritten. This came after the answer had been given" (L).

## Discussion of Results

The results show clearly that in the solution of problems and the answering of questions our O's had recourse to imagery. The most frequently occurring use is, as we might expect, that of imagery as the material for working the problem. Second in importance is the use of imagery as anchor. There were some instances (to be considered later) in which no imagery was used in the solution of the problems, but in nearly all of these cases the problem was anchored by imagery. It seems to be necessary to hold the meaning of the problem, or to fix its essential parts in some fashion, in order to answer it; and this is the use made of the anchoring imagery. The

attitudes included under "Regulation of problem" (3) probably occurred more frequently than they were reported. They are so largely meaningful that the underlying process easily escapes report. The meaning here is most often carried by verbal-motor imagery, though not infrequently other kinds of imagery do the work of formulation; and a visual image may correct a mistake or carry an anticipated meaning.

All of the imagery so far discussed is certainly relevant to the thought that it carries. We have, moreover, the statement of the *O*'s under the instruction to report "the experiences upon which the answer was based." Of a somewhat different nature are the five instances in which the imagery was used in aiding the *O* to evade the answering of the question. In three of the five instances reported the *O* was unable to answer the question, and so took refuge in imagery irrelevant to the problem-imagery. In the other two cases the answers were reached with difficulty, and the *O*'s allowed themselves to be side-tracked. We must note, however, that what we find here is not irrelevant imagery as such, but rather a shift to an attitude which is irrelevant to the problem-solving attitude. The imagery is relevant to the alternative attitude. The imagery which is illustrative of the answer (5) does not, of course, help in the solution of the problem. Its use seems to be the reassurance of the *O* that his answer is correct; and it carries in part the meaning of a feeling of satisfaction. In some instances it is purely associative. In any case the imagery is relevant to the attitude concerned.

There remain for consideration four phenomena reported by the *O*'s. These are:

	No. of cases
1. A felt need for imagery.....	6
2. Imagery as a hindrance.....	4
3. Irrelevant imagery .....	4
4. Cases in which the answer came immediately and automatically .....	20

We discuss these in the order above presented.

1. These instances show the dependence of the *O*'s upon imagery. The reports run as follows: *a*. "I was unable to get the hands of the clock changed in my visual image" (*L*). *b*. "I wanted to visualize and couldn't" (*S*). *c*. "This is difficult because I couldn't get a picture of the triangle" (*Sh*). *d*. "I tried to image the formation and couldn't, so that I used my fingers to help me out" (*L*). *e*. "I tried to visualize some paintings I had seen, and I couldn't" (*Z*). *f*. "I tried to get a visual image (to anchor the problem) and couldn't" (*S*). In 4 of the 6 instances (*c*, *d*, *e*, *f*) an incorrect answer or no answer at all was given. Of the 2 remaining cases, in *b* the *O* finally succeeded in evoking a kinaesthetic image which helped in solving the

problem; and in *a* the *O* reported that he could not be certain of his answer because of the incompleteness of the image which he had to use. These instances show that imagery not only is used when it is present, but also that its presence may be essential.

2. There were reported 4 instances in which imagery seemed to hinder rather than to help the *O*. Interestingly enough, though these include the reports of 3 different *O*'s, they are all of the same sort. The imagery which is reported as "being in the way" serves in every case as the anchor of the problem. Ordinarily, images serving this purpose drop out with the beginning of the solution of the problem, or become carriers of the processes involved in solving the problem. In 3 of the instances the anchoring images had the lure of familiarity, and were carriers of experiences more pleasing to the *O* than the duty of answering the question. This seems to be a form of "Means of escape from problem" (4). In the other case the *O* could not for a long time solve the problem, so that the anchoring imagery, which was very complete, was not replaced by any other imagery.

3. We have included under the heading "Irrelevant imagery" all imagery which was reported by the *O* as having nothing to do with the problem which he was solving. Since such reports bear directly on our main problem, we shall consider each one separately. *a*. Problem 6. Which is heavier, a pound of lead or a pound of feathers? Report: "A visual image of a flock of geese which some of my old neighbors used to have. There were several on a large pond and some on the bank. One spread out his wings and flapped them. This didn't help me solve the problem. The problem was solved at the end of the reading and this imagery came after the solution" (Z). The concluding statement of the *O* makes it clear that this is not a positive case of irrelevant imagery, but a case in which the imagery is relevant to a situation other than that involved in the solution of the problem. *b*. Problem 44. Can more than one meaning be attached to a sigh? If so, what meanings? Report: "As the phrase 'attach meaning' was completed, I thought of Titchener's 'Beginner's Psychology.' Then I thought of the next to the last lecture the first term. This was carried by visual imagery. . . . All of this bore no conscious relation to the answer given. The imagery came before the reading of the problem was finished" (L). It is evident that this imagery is used as an illustrative anchor which is anticipatory of the end of the problem. The last word of the problem, 'sigh,' demanded a shift in attitude so that the imagery reported is relevant to the attitude set up by 'meanings attached,' but not to the attitude which determines the answer. *c*. Problem 59. How would you criticize the following statement made by a judge to a prisoner: "You are to be hanged and I hope it will be a warning to you." Report: "A visual image of a young man, a round, ruddy lad. This is very clear. Other people are in the room. This is in the court-room of my home town. I think this is irrelevant [referring to the court-room]" (Z). We have here an example of imagery used as illustrative anchor. The irrelevancy, if there be any, is of the logical sort. *d*. Problem 6. Which is heavier, a pound of lead or a pound of feathers? Report: "The first thing that came to me was a remembrance of Prof. Angell, in his joking way, proving that psychologically feathers are heavier. This is not directly connected with the problem" (Sh). Again we find imagery used as an illustrative anchor, here in the form of a memory-image. It is difficult to see how this imagery is even logically irrelevant, since it obviously gives a clue to the answer.

4. A classification of the questions to which the answers came as sensorimotor responses throws light on the reason for this mode of response.

	No.
(a) Comparison problems .....	4
(b) Arithmetical problems .....	3
(c) Simple Judg. problems.....	3
(d) Gen. Informat. problems.....	6
(e) Easy Ingenuity problems.....	4

In the comparison problems the answer is dependent upon attention to the reading of the problem, for the answer is implicit in the statement. It may be necessary for some O's to restate the question in order to answer it, but for others the auditory perception touches off the answer. The questions asked of the types *b*, *c*, *d*, and *e* are of so simple and habitual a sort that the answers, having been previously worked out, are "on the tip of the tongue." In other words, all of the 20 cases are instances of the presence of brain-habit.<sup>9</sup> As we have said above, we frequently find in cases of this kind some anchoring imagery, but none which is used as material out of which the problem is worked; such imagery is not needed.

### Conclusions

I. We have shown (1) that, in solving a problem or in answering a question, imagery may be used in no less than 5 different ways; and (2) that in all cases the imagery reported is relevant to the thought whose meaning it carries —

II. During the course of the experiment there became obvious many imperfections in the method, which we shall now briefly consider. (1) *Difficulty in selecting the problems or questions.* The selection of problems or questions is by no means a simple matter. At first the O's were allowed to read the problems and to refer to them in the course of the solution. This procedure, however, did not permit of recording the time taken by the O to solve the problem or to answer the question. A time-record was deemed desirable as a check on the number of processes reported by the O, since he sometimes seemed to report experiences occurring, not in his solution of the problem, but during the period of introspection. We then tried reading the problem to the O and taking a time-record as described above. This proved to be a better procedure, but meant a change in the kind of problem used. A problem involving in its statement several terms or different steps, or a problem long in general, either could not be comprehended by the O or could not be held in mind from a single reading. Hence only those problems which could be simply and briefly stated and easily grasped could be used. Questions of general information were employed with the

<sup>9</sup> E. B. Titchener, *Thought-Processes*, 1909, 178 ff., 201.



hope of meeting this difficulty. It is not easy, nevertheless, to find questions which demand thought or present a real problem, and at the same time to avoid questions to which the answer comes automatically or to which the *O* can give no answer at all. There is, further, the task of selecting problems which shall not involve in their solution one type of imagery to the exclusion of others. The "clock" problems, the "folded and cut" paper problems, and the "box" problems are, for example, stated by Terman to test especially the ability to visualize.<sup>10</sup> The attempt was made to select problems appealing to other types of imagery as well. For example, we hoped that kinaesthetic imagery might be used in the solution of problems included in group 7. (2) *Difficulty in stating the problem.* We encountered a further difficulty in the statement of the problem. From the reports it was evident that the *O*'s were giving experiences set up by the descriptive part of the problem as well as those experiences upon which the answer was actually based. We were interested in the second form of report. To obviate this difficulty we tried to state the problems so that the important part for the answers should come at the end, as, for example: "What is a shoat?" (3) *Difficulty occasioned by the influence of the experimental attitude on the part of the O.* The *O*, having been informed in the instructions that he was to report the experiences upon which his answer was based, seemed in some cases to be disturbed by this requirement. This is shown by the fact (a) that some *O*'s "pondered" over questions which were simple, until there came to report something definite in the way of experiences which might underlie an answer reached almost immediately. For example, one *O* in his report on a "comparison" problem said: "The word 'oil' was articulated before spoken. I went through the problem in internal speech. The answer occurred immediately" (L). Yet the time recorded by the stop-watch was 14.4 sec. The tendency to wait for something "reportable" is perhaps the explanation of some of the illustrative imagery frequently reported. Some *O*'s delayed their answers until they were "ready to report," explaining the longer time thus required for the solution of the problem by remarks such as: "I was trying to think how I got the answer." Such a statement shows the honesty of the *O*, but makes the time-record valueless. The same thing is shown by the fact (b) that some *O*'s seemed to keep continually in mind during the solution

<sup>10</sup> L. M. Terman, *The Measurement of Intelligence*, 1916, 321, 328, 339.

of the problem that they were to report at the end, and accordingly introspected as they solved the problem. For example, one *O* said: "My introspections in this problem were separate from getting the problem. I have to answer the question and then go back to introspect" (*S*). Another illustration of the influence of the experimental attitude is the apparent fact (*c*)—*E* has no experimental proof except the time-records, which are otherwise difficult of interpretation—that in reporting the *O* sometimes added experiences then occurring to him, but experiences which were not a part of those upon which the answer was based. This, of course, is a trap into which it is easy to fall, and which can be avoided only by practice. It constitutes, nevertheless, one of the difficulties of the method, and involves the danger of assuming that the contents of the after-period are the same as those of the experimental consciousness. (4) *Difficulty in interpreting results.* The method puts the "burden of proof" upon *E*. His is the final interpretation of results. At best he can only check his interpretations by comparing the reports of different *O*'s and by repeating experiments. He is also aided by interpretations which the *O*'s sometimes "let slip."

That the method lacks the accuracy of other experimental methods is clear. Nevertheless, as a starting-point in an experimental investigation, it is valuable. "It will always be of service where new ground has to be broken, and where the formations are so complex that an immediate recourse to experiment in the strict sense is forbidden."<sup>11</sup>

#### EXPERIMENT A. 2. REPETITION OF FOX'S EXPERIMENT

With a view to further study of method and interpretation of results we undertook the repetition of an experiment reported by Fox in the *British Journal of Psychology*.<sup>12</sup> His problem and method bore some resemblances to ours in the experiment described above, and we hoped that a comparison of our results (from the repetition of the experiment) with his might throw additional light on both the method and interpretation of results. We shall state briefly his procedure and the general results of his experiment and then give the conditions of our repetition of the experiment and our results.

Fox's subjects were told that "they were to investigate the existence and importance of thought without images, and to try to find out the content of such thinking."<sup>13</sup> They were also "to distinguish, as far as they could, between the thinking and the thought."<sup>14</sup> He worked with

<sup>11</sup> E. B. Titchener, *The Method of Examination*, *Am. Jour. Psych.*, 24, 1913, 429 ff.

<sup>12</sup> C. Fox, *The Conditions which Arouse Mental Images in Thought*, *Brit. Jour. Psych.*, 6, 1913-14, 420 ff.

<sup>13</sup> *Op. cit.*, 420.

<sup>14</sup> *Ibid.*, 420.



5 subjects, all of whom had had "previous practice in introspection."<sup>15</sup> The material used was 12 statements: 4 involving mathematical conceptions, 4 historical, 3 grammatical, and 1 a couple of lines from Milton. What the instructions were is not clear. We know only that "the subjects were told to record on a sheet of paper everything they could discover by introspection after each statement had been read twice by the experimenter. They were told to put down everything, however unimportant it appeared to them. . . . In fact all details, whether mental or physical, were to be noted. They were also told to put down first of all whether they realised the meaning of the statement read to them; and as soon as the meaning was realised the process of thought which had led to its realisation. If possible they were to state what the realisation consisted of; and whether it involved mental images or not. In cases where images did arise they were instructed to state whether the realisation of meaning preceded or succeeded the occurrence of the mental image."<sup>16</sup> Fox states later, however, that "these instructions were only fully carried out as regards that part of them which related to the realisation of meaning and to the occurrence of images."<sup>17</sup> The general results of the experiment he states as follows: "any delay or conflict in consciousness is a favorable condition for arousing a relevant mental image. . . . The experiments also show directly that the contrary set of conditions are (*sic*) unfavorable to the production of images."<sup>18</sup> In repeating the experiment we used the same material, though the statements were read by *E* only once. Our instructions differed from those of Fox in the following respects. (1) We did not tell our *O*'s that they were "to investigate the existence and importance of thought without images."<sup>19</sup> (2) The reports were made orally by the *O*'s. (3) The instructions were less full and suggestive than those used by Fox, in accordance with his statement that his own detailed instructions "were only fully carried out as regards that part of them which related to the realisation of meaning and to the occurrence of images."<sup>20</sup> Since we were not certain whether Fox meant the same thing when he asked his subjects "to state what the realisation consisted of"<sup>21</sup> and when he told them to give "the process of thought which had led to its realisation," we used two sets of instructions which read as follows. (1) "I shall read you a statement. Please report whether you realise the meaning of the statement read and, if so, report the process of thought which led to its realisation." (2) "I shall read you a statement. Please report whether you realise the meaning of the statement read and, if so, of what the realisation consists." The 12 statements of Fox were used as material with the first set of instructions; but in order to make a comparable situation we were forced to use different statements for the second set of instructions. We endeavored, however, to find sentences equal in difficulty and similar, so far as possible, to those used by Fox; that is, 4 were mathematical, 4 were historical, etc. We had 5 *O*'s, 3 of whom, S, H, and Z, had acted as *O*'s in our first experiment. The

<sup>15</sup> *Ibid.*, 421.

<sup>16</sup> *Ibid.*, 421.

<sup>17</sup> *Ibid.*, 421.

<sup>18</sup> *Ibid.*, 430.

<sup>19</sup> *Ibid.*, 420.

<sup>20</sup> *Ibid.*, 421.

<sup>21</sup> *Ibid.*, 421.

would not be used as are the images which appear as the result of a conflict. For example, in reporting on this same sentence, one of our O's (H) said: "'Mechanical inventions' gave rise to auditory imagery meaning 'Edison' and visual imagery in greys of different brightness that meant 'transportation' or 'industrial activity.' I seemed to have settled the thing until the word 'England' came. With this the visual image dropped out. Then an auditory image of the word 'England' that meant to assure myself of what you said. Then followed a bit of visual imagery in greys that referred to conditions in England and meant the translation of the previous meaning from this country to England." 3. "A strong image may obstruct the attempt to understand."<sup>32</sup> Fox does not define 'strong,' so that we do not know whether he refers to clearness, or to details, or to stability and duration. His illustrations of "certain cases" in which the image obstructs understanding do not, however, seem conclusive. In any case they do not prove, as perhaps they are not intended to prove, that thought may be imageless. One of Fox's subjects reports: "When I tried to realise the significance of the statement it was twice obstructed; at first by the picture of my old history room at school, then by my history book open at the page on feudalism."<sup>33</sup> This seems to be an example of our anchoring imagery. It is true, of course, that attention to these images for themselves would involve a shift in attitude, and that they would thus prove an obstruction to realisation of meaning. 4. In considering the propositions of the third group, Fox states that "prompt and thorough understanding coincides with the absence of images."<sup>34</sup> This is evidenced by the reports of three subjects of whom Fox says: "Three subjects obtained what may be described as an associative image, namely an image not directly called up by the lines but evoked by association with their meaning. In these cases the image was that of a book on education in which a similar doctrine to that expressed in the lines was discussed. Now those who had these images must have realised the meaning before the images came, since such images depend on understanding the meaning."<sup>35</sup> Hence he concludes that these three subjects realised the meaning of the statement without the aid of images. We have not the introspective reports for reference, and it may be that these present evidence that the images were associative. From what Fox tells us, however, it does not seem necessarily true that "those who had these images must have realised the meaning before the images came, since such images depend on understanding the meaning." It seems quite possible that the image of "a book on education" might have carried the meaning of the lines instead of being dependent on them. In conclusion Fox states: "The experiments show that any delay or conflict in consciousness is a favorable condition for arousing a relevant mental image, that is, one that will in some way tend to help towards a cessation of the conflict. All the other conditions which we have found to be suitable for stimulating the production of mental images are reducible to this general formula. . . . The experiments also show directly that the contrary set of conditions are (sic) unfavorable to the production of images. Thorough or imme-

<sup>32</sup> *Ibid.*, 427.

<sup>33</sup> *Ibid.*, 427.

<sup>34</sup> *Ibid.*, 429.

<sup>35</sup> *Ibid.*, 429.

ate understanding, an easily grasped conception, ready assent to a proposition, straightforward or unimpeded reasoning, are all cases in which, as a general rule, images play no part. Further, concentration of thought on meaning is unfavorable to the stimulation of mental imagery, but this cannot be brought easily under the above general formula."<sup>36</sup> We agree with Fox's statement with respect to the conditions favorable for arousing mental imagery. We cannot, however, fully accept his statement of conditions unfavorable to the stimulation of imagery. "Thorough or immediate understanding," in cases where the propositions come to the *O* labeled "You've accepted me before," may be explained by the operation of brain-inhibition, so that no imagery would be needed. In "straightforward or unimpeded reasoning" it seems probable that, the attention being on meaning, the processes might be overlooked. Of the 60 thought-processes of our 5 *O*'s, only 3 or 5% occurred without imagery. We are, accordingly, inclined to believe that, in some at least of the situations mentioned by Fox as unfavorable to the production of imagery, the imaginal content was overlooked, so quick is the process of thought and so completely is the attention of the subject likely to be concentrated on meaning. We have a parallel case in the neglect of inter-images and double images in everyday experiences when other things are in the focus of attention. We should, therefore, agree with Fox's last statement (if we may change the word 'stimulation' to 'report') that "concentration of thought on meaning is unfavorable to the report of mental imagery."

The general results of the experiments in which we used the second set of instructions and material similar to the statements of Fox may be briefly mentioned. With the exception of one *O*, D, these instructions were interpreted as calling for a report both of meaning and of process. This *O* persisted in his statement that he was never aware of the realisation" and so could not report on it. The reports with these instructions were much less full, and the use of the expression "of what the realisation consists" seemed to throw the emphasis on meaning rather than on process. One *O* (as we have reported above) said: "To me realisation means a sort of acceptance or approval of the meaning." If Fox's subjects interpreted his instructions in this way, it is evident that they were set to report meaning and not process; so that images, although present, may easily have been overlooked.

Two things may be learned from the repetition of this experiment: (1) the necessity of phrasing instructions carefully and accurately; (2) the need for as flexible and unbiased an interpretation of results as possible.

#### EXPERIMENT B. 1. LIKENESSES AND DIFFERENCES

Our problem remained the same as in Experiment A. In considering the results of this experiment we discussed some of the difficulties encountered in arranging material and determining procedure. In Experiment B we tried to overcome some of these difficulties. Our material consisted of paired words whose relations to each other with respect to likeness, difference, etc., the *O*'s were to give. It may be remembered

<sup>36</sup> *Ibid.*, 430 ff.

that we found that one of the sources of error in the material used in Experiment A was the lengthy statement of the problem. We hoped that the series of paired words would eliminate this difficulty; the question asked in the instructions would finally be present only as a set, and the statement of the problem would be reduced to two words. It seemed also that this type of material might be a halfway house between the "wordy" problem and the single word-stimulus.

The *O* was seated with his back to *E*, and was instructed to keep his eyes closed during the reading of the paired words and the determination of the answer. The instructions were as follows: "I shall present to you a series of paired words and I want you to tell me in what respect the members of each pair differ from each other. After you have done this, please describe as well as you can the experiences upon which your differentiation was based." Three series of 15 paired words each were used. The two words were read to the *O*; the stop-watch was started as the last word was spoken by *E* and stopped as soon as *O* gave his answer. The following are typical of the paired words used; cut-scratch, education-culture, possible-practicable, Dickens-Scott. There were five *O*'s. Two of these, *Z* and *H*, had worked in Experiment A. The others were Miss R. Stutsman (*St*), and Messrs. P. Cavanaugh (*C*) and E. Tolman (*T*). *C* had had some training in observation, but *St* and *T* were untrained. The experiment covered a period of two weeks. At the end of this time the *O*'s were asked to give likenesses instead of differences, and the instructions were changed accordingly. Thirty pairs of words, such as memory-imagination, water-air, nymph-mermaid, were used.

Uses	Number of Cases				
	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>
1. Anchor					
( <i>a</i> ) Illustrative.....	57	93	6	12	7
( <i>b</i> ) Non-illustrative.....	18	24		2	
( <i>c</i> ) Combination of <i>a</i> and <i>b</i> .....	12	8			1
2. Subject-matter of Problem					
( <i>a</i> ) Changing.....	42	90	12	16	12
( <i>b</i> ) Fixed.....	6	3		6	
( <i>c</i> ) Combination of <i>a</i> and <i>b</i> .....		1			
3. Regulation of Problem					
( <i>a</i> ) Formulation.....	9	8			2
( <i>b</i> ) Anticipation.....					
( <i>c</i> ) Criticism.....	2	3			
4. Means of Escape from Problem					
5. Illustration of the Answer.....	6	3			2
( <i>a</i> ) Reinforcement.....	3				

*a*, *b*, etc., at the head of the Table, refer to the relationships listed on p. 213.



The experiment covered a period of a week. In order to eliminate certain difficulties, which we shall discuss later, we again changed the material and instructions. We still used paired words, but words which stood in various relations to each other. The instructions were: "I shall read you 2 words. You are to tell in what respect (a) they are like each other, (b) they differ from each other, (c) one is dependent upon the other, (d) one is convertible into the other, (e) one affects the other. After you have given your answer, please describe the experiences upon which the answer was based." Before pronouncing the 2 words, *E* indicated the relation between them to be given by *O*. The following are typical of the paired words used in this series: hypothesis-law (convertible), democracy-education (affect), capital-labor (dependent). The experiment lasted 2 weeks and with one exception (*H*) the *O*'s were the same as before.

We give first a classification of the imagery used in determining the various relations between the paired words.

The types of imaginal experience upon which the differentiations were based are:

1. Two images which may be present (a) simultaneously, (b) successively, (c) successively with a recurrence of the first image. Number of instances=104.

Stimulus: egg-stone. Report: "One is breakable, the other breaks. There was a scrappy bit of visual imagery, vague. It carried the meaning of falling and breaking. Over against that was a kinaesthetic image in the throat. It carried the meaning of hard, something that couldn't break" (*H*).

2. Two images may be present and the basis for differentiation be carried by the second. Number of instances=13.

Stimulus: stumble-fall. Report: "To fall is to go clear to the ground. A visual image of myself stumbling on a brick walk. I was leaning forward. There was a weak kinaesthetic image in my chest and shoulders and a motor image in my toe. Then the word 'fall' came. With this there came, in just a flash, a visual image of a man lying on the ground. Then the difference came; to fall is to go clear to the ground" (*Z*).

3. One or two images may be present, the differentiation being made by verbal-motor imagery. Number of instances=27.

Stimulus: cook-fry. Report: "You boil in water, but you fry in oil. I had a visual image of something frying in a skillet. With 'cook' there was a visual image of a pot. I was trying to get at something in the pan and pot to differentiate them. This was in verbal-motor imagery: 'You don't fry in water'" (*Z*).

4. There may be a visual schema, accompanied or unaccompanied by anchoring images. Number of instances=15.

Stimulus: radical-progressive. Report: "A radical person has less sense than a progressive person. A visual image in dark grey of the two words written out on a neutral grey background. 'Radical' was above 'progressive.' At that point there was no difference. Then 'radical' moved rapidly to the right. The right meant 'ahead of,' 'way from,' or 'his going too far ahead to be sensible.' All this pushed the answer off" (*C*).

5. There may be two modalities carrying the meaning of the first stimulus-word, the differentiation being based on the absence of one of the modalities in the imagery carrying the meaning of the second stimulus-word. Number of instances=7.

Stimulus: fry-cook. Report: "The difference is auditory. With 'fry' I had a vague visual image which meant a frying pan with meat on a gas-plate. This was very scrappy, and was accompanied by a clearer auditory image of equal intensity. With 'cook' there was a vague visual image of a pot. I saw nothing cooking in it, though the meaning was there. The auditory image was lacking here, and this lack forced the first difference" (H).

6. There may be verbal-motor imagery accompanied by illustrative imagery. Number of instances=3.

Stimulus: powerless-weak. Report: "You may have a little power and be weak, but if you have no power you can't do anything. I was at sea for a while. Then I thought (verbal-motor imagery) 'They're absolutely synonymous, but she asked for a difference.' Then I thought of an engine, an automobile engine, powerless; then of an engine running very weakly (visual imagery) and then came in the concrete experience which was the basis of differentiation" (Z). There were reported 3 instances in which the differentiations were based on different feelings (pleasantness and unpleasantness) set up by the two stimulus words; two instances in which the differentiation was based on two different sensations; and two in which it was based on a difference between image and sensation.

The types of imaginal experiences upon which the likenesses were based are:

1. Two images may be present (a) simultaneously, (b) successively, the likeness being determined by the presence of one or more similar qualities in the two images, or by full similarity, or by eye-movement from the one image to the other; or the answer may be set off immediately.<sup>37</sup> Number of instances=42.

2. There may be two images, the first of which carries the meaning of a superordinate class. Number of instances=16.

Stimulus: purple-orange. Report: "Both are colors. After the stimulus 'purple' I had a visual image of a patch of dark purple. With 'orange' the verbal-motor image 'color' came. The purple image meant 'color,' so that when 'orange' came I could give 'color' at once" (Z).<sup>38</sup>

3. There may be a visual schema. Number of instances=3.

4. There may be anchoring imagery together with verbal-motor imagery. Number of instances=12.

5. Illustrative imagery may follow or accompany a response given immediately. Number of instances=9.

Stimulus: apple-ball. Report: "Both are round. As soon as I had said 'apple-ball,' 'round' came. After I had made my decision visual images of 'apple' and 'ball' came" (Z).

<sup>37</sup> Illustrations will be given only in cases in which the types of experience differ from those reported for the differentiations.

<sup>38</sup> Cf. the "übergeordneten Begriff" of A. Messer, *Experimentelle psychologische Untersuchungen über das Denken*, *Arch. f. d. ges. Psych.*, 8, 1906, 78.



The analysis of the experiences upon which the determinations of the other kinds of relations were based showed no new types of imaginal experiences.

### *Conclusions*

I. We have obtained in this experiment further evidence of the importance of imagery. Although we reduced our problems to lowest terms, imagery was needed. Almost as important as the imagery is the part played by the "set" produced by the instructions. It will doubtless have been observed in the giving of both likenesses and differences that the presence of two images carrying the meanings of the stimulus-words was not enough in itself to touch off the answer. The determination to report a difference or a likeness was the essential thing. This is especially noticeable in the case of the differentiations.

II. Our material and procedure in this experiment were arranged with a view to eliminating some of the difficulties found in Experiment A. (1) *Material*. The shortened statement of the problem avoided the setting up of imagery present in the reading of a longer problem, and focused the attention on the question itself. This is clearly advantageous. On the other hand, unless the stimulus-words are chosen with care, much less thought is required than in the case of the problems. The acquisition of a set for a superordinate concept, as was the case with the likenesses, made *O*'s task considerably easier. The simplicity or perhaps the uniformity of presentation of the material seemed to make possible a fixed kind of reaction to it. The generality of the instructions permitted superficial answers and, accordingly, less thought on the part of *O*. The demand for a more fundamental likeness or difference meant the suppression of the first answer that came and the search for a second; all of which would increase the task of report. (2) *The task of the experimenter*. In the interpretation of results *E*'s work was much easier with the use of this material. The reports of the *O*'s were briefer and seemed to be more accurate and complete. The *O*'s were often able to say with reference to a specific process: "It was this that carried the meaning of likeness," thus marking it off from the other processes reported. If the *O*'s are to be trusted, *E*'s burden is appreciably lightened. (3) *The influence of the experimental attitude upon the O's*. We have already mentioned the fact that *O*, knowing he has to report his experiences, may be set to think in imaginal terms to a greater extent than is usual with him. Indeed, one of our

O's, after having given a full report of the imagery used in making a differentiation, said: "If I were asked on the street to give the difference between "old" and "obsolete," I think that the thing would go off in verbal terms." This remark led us to undertake our next experiment.

#### EXPERIMENT B. 2. EXPERIMENT UNDER NON-LABORATORY CONDITIONS

We wished to find to what extent imagery is present as a basis for answering questions when the questions are not a part of a laboratory exercise. In other words, we wanted to catch our O's without the experimental attitude upon them, and without the knowledge that they would be asked to give "the experiences upon which the answer was based." Outside of observation-hours questions such as would arise naturally in the course of a conversation were asked casually. When the "O" had as casually answered, we asked him if there were any imagery present as the basis of his answer. We have 29 reports from 5 of our regular O's, Z, H, B, D, and S; and 4 reports from persons having no training in psychology. These reports are, of course, necessarily incomplete; but with only 3 exceptions there was reported imagery of some kind upon which the answer was based. In 2 of the 3 exceptions kinaesthesia was reported; and in the case of the remaining exception the subject stated that the answer came automatically. We give an illustration of the procedure used. The "E" and the "O" were talking about Stout and his books. O was asked what Stout had written, and replied: "He wrote a couple of books that I know of and he stands for the theory of conation." E then asked: "Did you have any imagery when you answered my question?" The answer follows: "Yes. I had a visual image of the titles of the books and of their covers. I also saw printed, as if an excerpt from a book, the word 'conative.'" This group of experiments shows that imagery is of frequent occurrence in everyday thinking. The high percentage of cases in which imagery was reported is undoubtedly due to the fact that 5 of the 7 persons questioned were trained O's. The demand for a report under these conditions implies a quick shift from a logical to a psychological attitude, of which only trained O's are capable. It is clear, then, that a good deal of imagery is present in thinking, and that its presence is not solely the result of laboratory conditions.

#### EXPERIMENT C

Our experiments in thinking gave us no instances of irrelevant material. Though we had been attacking our problem on the side of relevancy, we had expected to get at irrelevancy indirectly. It seemed best now to concern ourselves directly with irrelevancy. We began this section of our experimental work with experiments at the perceptive level, resembling everyday experiences. Each one of the situations in these experiments included an irrelevant factor. We wished to obtain a description of consciousness at the instant when the irrelevant factor appeared in the situation.

There were 3 parts of the experiment. (1) We set the *O*'s simple tasks into each one of which an irrelevant factor was introduced. At the appearance of the irrelevancy we interrupted the *O* in his task, and asked him to describe his experiences at the moment of interruption. There were 4 tasks of this sort: (a) the sorting, according to 3 presented samples, of buttons drawn from a bag in which was one button unlike any of the samples; (b) the sorting, according to the arrangement of holes punched in them, of cards taken from a piled pack, one of which was different from all the others; (c) the arranging, in order of preference, of samples of cloth pasted on paper, among which there was an oblong of paper with no cloth on it; (d) the presentation, for brief study, of a picture about which were later asked questions one of which had nothing to do with the subject-matter of the picture. The instructions were: "Here are 3 buttons, and others of the same sort are mixed up in the bag. You are to put your hand in the bag and, taking out one at a time, arrange them in piles according to the samples shown. When I say 'Now,' you are to drop the task if it is still incomplete and describe your experiences at the moment of interruption." The first part of the instructions was changed to suit the nature of the task, but the latter part remained the same for all 4 tasks. The time was taken with a stop-watch chiefly in order to hold the *O* strictly to his task.

(2) The material for the second part of the experiment was a paragraph the latter part of which was entirely irrelevant to the beginning. The irrelevant part was begun on the last line of a sheet of paper so that the *O* was obliged to turn to the second sheet to continue his reading. This arrangement had the double advantage of preventing the reader from glancing ahead to the irrelevant part, and of letting *E* know when to interrupt. The instructions were: "I shall give you a paragraph which I want you to read carefully enough to give its contents after reading. When I say 'Now,' you are to drop the task if it is still incomplete, and describe your experiences at the moment of interruption." (3) The material for the third part of the experiment consisted of 4 paragraphs which contained such errors as: repetitions, omissions, misspellings, transpositions, and omission of punctuation. The instructions were: "I shall give you a short paragraph which you are to read carefully. After you have finished your reading, you are to give a summary of the paragraph." After the *O* had read and reported on all 4 of the paragraphs, he was asked if he had noted any errors of form, and if so to describe his experiences when he noted them. Only one of the foregoing experiments took place in any one observation-hour, and it either preceded or followed other experimental work. There were 5 *O*'s, 3 of whom, H, S, and Z, had observed in Experiments A and B; and 2 of whom, B and D, had observed only in the repetition of Fox's experiment.

### *Sorting Problems*

Three of the tasks (1 (a), (b), (c)) were of the same general nature, *i.e.*, sorting problems. The reports show the presence of 4 stages in the realisation of irrelevancy:

1. Perception (usually tactual), accompanied by immediate judgment of difference and supplemented by imagery;
2. Visual perception of difference;

## 3. Feeling component:

- |                         |   |
|-------------------------|---|
| (a) Astonishment        | } Analyzable into kinaesthetic sensations, affective processes, and imagery |
| (b) Surprise and wonder |   |
| (c) Indecision          |   |

## 4. Reaction:

- (a) Immediate (directed by determining tendency);  
 (b) Mediate (verbal-motor imagery).

We give a report of the experiences described in the performance of one of the tasks. "First a cutaneous perception which was different from the preceding one; the pressure was much heavier. There was a brief snatch of visual imagery which went with this cutaneous pressure sensation and carried the meaning of the type of button and the fact that it was different. I think I verbalised this as: 'I wonder what this is.' Added to this was the perception of the visual black [the odd button was black], and then without consciously intending to do anything, I threw it outside. The kinaesthetic process and the perception of throwing brought the meaning of rejection" (D). In the case of some of the other O's the feeling-part of the experience was more marked. It was always present, and seems to be characteristic of the appearance of the irrelevant component of the situation.

The sorting of cards, as an experience containing an irrelevancy, was a failure. The card which differed in the arrangement of its punched holes from the other cards was in no case perceived as different, the difference apparently being so slight as to be overlooked. The reports on this situation are, however, valuable; they show the difference between a situation containing an irrelevant factor and one containing only relevant factors. We find reports of stage 2, the visual perception of difference, and of stage 4, the reaction. In this perception there was no tactual element, for the 'feel,' in general, of all the cards was the same. The significant thing is the absence of the feeling-component. Of importance, also, is the fact that in all cases the reaction was immediate, never mediate.

*Reading Problem: Irrelevant Meanings*

In their reports on the second part of the experiment, in which the material was the single paragraph, all O's noted the "meaninglessness" of the irrelevant part. The absence of meaning was accompanied by feelings of strangeness, confusion, puzzlement, etc. The meaning of irrelevancy was carried by pressure sensations (staring at the page), muscular tension (an attempt to hold the meaning together), and verbal-



motor imagery, which usually expressed an attempt to force a relationship to the preceding part of the paragraph.

#### *Reading Problem: Typographical Errors*

In the third part of the experiment we set our *O*'s too difficult a task. They could not give a report of processes, even when they had noted errors, so long after the observation had taken place. The difficulty was further increased by the set for relevant meanings. It should be said, however, that we were aware of the difficulty of this task; we had wished to arrange an everyday situation, like the reading of a newspaper, in which the irrelevancies should be commonplace. One *O* noticed no errors and said reproachfully when asked to report: "I didn't notice any errors; I wasn't set for proof-reading" (*Z*). The 4 other *O*'s remembered and reported mistakes in the paragraphs, but 2 of them could give no report of process because they had not taken the attitude for report. The third *O*, *H*, reported that he ignored the errors as he read, "for he was reading for thought rather than for grammar." He mentioned, however, a tendency to read aright in auditory imagery the mistakes, and also some verbal-motor imagery carrying the meaning of errors. The fourth *O*, *B*, reported verbal-motor imagery carrying the meaning of annoyance at writing so carelessly done by *E*.

#### *Conclusions*

We have seen that a marked irrelevancy is characterized by feeling accompanying the inhibition set up; and that where the irrelevancy is less marked, it tends to be overlooked because of the set for relevant meanings.

#### EXPERIMENT D. 1. PICTURES

In Experiment C we began to study irrelevancy directly. We arranged situations containing irrelevancies on the perceptive level, and we secured an analysis of the situations from our *O*'s. From these analyses we found that when an irrelevancy appeared in a situation it was accompanied by "feelings." We wished now to find out whether such feelings might be the criteria of the entrance of irrelevant imagery into an imaginal situation. To this end we had to arrange a situation in which there should be some imagery irrelevant to it; as, for example, the black button was irrelevant to the other buttons in the bag. We wanted, with our



*O*'s set for imagery, to have a complex background of imagery upon which they could draw. To secure this we used 3 kinds of material and procedure.

1. We presented to *O* a series of 12 pictures, mainly narrative, brightly colored, and varying in size from 6 by 8 inches to 12 by 16 inches. The instructions were: "I shall show you a picture for a short period. Please observe it carefully. At the end of this period you will be allowed two minutes in which to write a description of the picture, giving its title." The *O*'s were seated in front of a grey screen upon which was mounted one of the pictures. The picture was at first covered by a curtain, which was raised just after the "ready" signal and let fall after an exposure of 15 seconds. This procedure took one observation-hour. At the beginning of the next observation-period we gave the *O* the following instructions: "I shall name to you one of the pictures that you learned last time. When you hear the name, I want you to recall the picture as vividly as possible. Three seconds after I have named the picture I shall read you a simple problem which you are to solve as quickly as possible. Say 'Yes' when you have solved it, and then begin to report as fully as you can the course of your experiences during the experiment." After each one of the pictures had been presented once for recall, they were presented again in a different order, and followed by different problems.

2. The material for the second part of the experiment was put in the form of a completion-test. We took from various books descriptive paragraphs having the following titles: "The Mountains of the Desert," "Rules of Hunting among the Greenlanders," "Daybreak" (poetry), "A Japanese Garden," "The Winter Dwellings of the Esquimaux," and "A Simple Chronoscope." Several words, including all parts of speech, were omitted from these paragraphs. The *O* was given the following instructions: "I shall give you a paragraph in which some of the words are missing. You are to fill in the blank spaces, each of which indicates an omitted word. You will be given a certain length of time in which to do this. At the end of this time I shall say 'Now' and read you a problem which you are to solve. After you have given your answer, report all of your experiences from the reading of the problem to the giving of the answer." No fixed period of time was allowed for filling in the words of the paragraph. The *O* was interrupted in his task when he was seen to be near the end of the paragraph.

3. The material for this part of the experiment consisted of 3 separate words. For the most part the words all referred to a single situation as crowd—touchdown—cheers; though there were a few cases in which the 3 words might set up different trains of thought, as for example turkey—star—electric. The instructions show the procedure. "I shall read you 3 words. Give yourself passively to any imagery that comes. After 15 seconds, I shall read you a problem which you are to solve and report on in the regular way." *O* was seated with his back to *E* and with his eyes closed. There were in the three parts of Experiment D 5 *O*'s, all of whom had served in the other experiments.

In the first part of the experiment we endeavored to supply the *O* with a background of imagery upon which should be

impinged a problem to be solved. We desired, of course, to know what happened to this imagery when the problem came. We shall refer to the imagery called up by the *O* in this part of the experiment as the picture-imagery, since it was determined by the previous presentation of pictures. An analysis of the reports of the 5 *O*'s shows that the picture-imagery may behave in at least 3 different ways.

(1) It may drop out at the beginning of the reading of the problem, either suddenly or gradually.

Report: "The first imagery (picture-imagery) came quite quickly, so that when I repeated the name of the picture, it was there almost immediately. In quality it was greyish and reddish. It lacked a background. Then began the reading of the problem, and there was with it rather good visual imagery of trees. Somewhere here the first imagery went out" (D).

(2) It may remain during the reading of the problem, in which case the problem is (a) not sensorily clear, (b) not cognitively clear, (c) neither sensorily nor cognitively clear, and the picture-imagery loses in clearness, detail, meaningfulness; or there is a fluctuation in clearness between the picture-imagery and the problem, in which both seem equally insistent in turn.

Report: "The auditory stimulus (name of picture) carried the meaning of familiarity and with this there was a visual image of the bird sitting on a post out in Oregon where I knew the bird. Then quickly came the image of the picture as seen. The image was intense and rich in detail. Then the problem came. It was a sensation without meaning. The association with the bird persisted; its song came in auditory imagery and a visual image that meant 'bird' persisted all the time that the words of the problem were coming. It was there until nearly the end of the reading. During the last part of the reading the attention was on the auditory sensation more than on the meaning. Then an auditory image that meant the whole problem repeated twice. By this time the 'bird' was pushed aside and the attention was on the problem. The meaning of the problem became clear cognitively" (H).

(3) It may remain or may recur during the reading of the problem and be illustrative of it.

Report: "The auditory sensation of 'The Line Up' (the name of the picture) brought a visual image that meant the picture. It was rich in color-meaning. At first the auditory sensations were very obscure, not cognised. At the third word there was touched off a determination to solve the problem, and the visual imagery dropped right out. With the word 'cloth' (word of the problem) there was a recurrence of a bit of the picture-imagery meaning the red shawl on the man's back. From there on there was a determination to listen to the problem" (H).

What happens to the picture-imagery is obvious. Where it is irrelevant, it drops out; where it can be used,—that is, where it becomes relevant,—it remains. It is true that it may not drop out all at once; but in that case it interferes with the grasping of the statement of the problem, as well as

being interfered with in its turn by the problem. There is a struggle between the two for the field of attention, and now the one and now the other is clear. There is, moreover, no awareness of irrelevancy so far as the picture-imagery is concerned. The irrelevant factor here is the auditory perception of the problem, and we find its appearance characterised in much the same way as was the appearance of the black button. For example, one *O* reports: "I couldn't shake the picture out of my mind (when the problem came). There was muscular tension and internal speech while I was trying to get my bearings. There was strain around the eyes, discomfort, and unpleasantness" (B). This is evidently our feeling-component of Experiment C; but here, as there, it is characteristic of a perception, not of an image. We have another kind of situation when the imagery remains or recurs during the reading of the problem. In this case the shift from the picture-imagery-awareness to the problem-awareness has been made. In two of the three reported instances of this sort the picture-imagery has entirely faded out, and recurs only to carry the meaning, in an illustrative fashion, of part of the problem. It is to be noted that only that part of the picture-imagery which is relevant to the problem-meaning recurs; the whole of the image does not come back. We have thus succeeded in giving our *O*'s a background of imagery, and they draw upon this where they can; for the rest, it disappears. In the third instance included under this heading, the picture-imagery remained during the reading of the problem. It began, however, to lose in detail, and now one part and then another of the visual image was clear. The problem was lacking in cognitive clearness. The first word of the problem to be comprehended was the word 'trees' which the *O* said "persisted" and was related to a tree in the visual image. What we have here is a reinforcement of the problem-meaning by a part of the picture-imagery. After this the picture-imagery dropped out and the problem became clear. There remain for consideration 3 cases in which the problem-imagery was superposed on the picture-imagery. These we shall discuss along with the results of the third part of the experiment.

#### EXPERIMENT D. 2. COMPLETION-TESTS

We expected in this part of the experiment, as in the preceding part, that the *O*'s might be "revelling in imagery" when the problem came; and we hoped that they might have recourse to imagery in their attempts to fill in the blanks

the completion-test. We interrupted them just before the paragraph was completed, so that any imagery present should have been at its richest. An analysis of the reports shows the experiences of the *O*'s when the problem came and the effect of the previous task on the problem.

1. There may be a complete and immediate shift in attitude from the completion-test to the problem.

Report: "In the paragraph the first blank was a nuisance to me. I was tired of the test and in a sort of careless attitude so that when the problem came I was glad; and the test dropped out entirely" (*Z*).

2. There may be between the test-attitude and the problem-solving-attitude a state characterised by the *O*'s as "a period of cognitive blankness" or as "a chaotic consciousness."

Report: "Consciousness was chaos at first. There was a series of auditory sensations having no meanings" (*S*).

3. The test-attitude, carried by (*a*) visual imagery, (*b*) kinaesthetic imagery or kinaesthetic sensation, renders the problem sensorily unclear, cognitively unclear, or both.

Report: "Visual imagery (from the completion-test) was present when you started reading the problem. As the problem was read I turned sharply toward you, meaning that I was going to pay attention. The first part of the statement came merely as auditory sensation and without specific meaning, though with the meaning 'here is the problem.' There was a reference back to the paragraph carried by kinaesthetic sensation; a tendency to turn the head back. Then there was verbal-motor imagery of parts of the problem and some visual imagery" (*D*).

4. The imagery used in the completion-test may hang over or recur during the reading of the problem, in which case the problem is sensorily and cognitively unclear or is not heard. The results of this experiment show that the conditions set are not conducive to the "carrying-over" of imagery from one situation to another; the break between the two situations is too great. What we really have here is a study of shift in attitude. There remains for consideration one case in which the imagery used in the completion-test remained during the solution of the problem. This we shall discuss in connection with the results of the third part of the experiment.

Just as in the first part of the experiment the reading of the problem was characterised by a feeling-component, so was the solution here. The *O*'s were "annoyed," "irritated," "bothered," when the problem was read and the set task interrupted. They are again reminded of the experiments in perception.

#### EXPERIMENT D. 3. THREE-WORD IMAGERY

In the third part of the experiment the procedure was more successful so far as the setting up of a background of imagery is concerned. We have a total of 324 reports from the *O*'s. These show that the imagery set up by the 3 words to be referred to as the 3-word imagery) may behave as follows:



1. The imagery may drop out before the problem comes, or with the reading of the first word. Number of instances=79.

Stimulus: bells—horses—fire. Report: "I was trying to hear the bells ring when the problem came. This was carried by strain in the ears. This dropped out at once and the problem was the only thing in mind" (B).

2. It may remain during the reading of the first few words of the problem; or during the whole of the reading of the problem, in which case the problem may be rendered sensorily or cognitively unclear. Number of instances=79.

Stimulus: elephant—man—sawdust. Report: "The 3-word imagery was good. When the problem began I did not attend, for I was engrossed with a visual image of the elephant and his trainer and the sawdust on the floor. Then I 'picked up' a memory after-image of the first part of the question and the visual image dropped out" (Z).

3. It may recur or it may fluctuate with (a) the reading of the problem or (b) the solution of the problem. Number of instances=46.

Stimulus: June—stars—perfume. Report: "The 3 words set up visual imagery and also some auditory and olfactory imagery. The problem at first was obscure. The imagery persisted clearer than the sensation. Then the imagery dropped out and the sensation became clear cognitively. The visual imagery came back very, very briefly. There was no connection of meaning between the visual imagery and the auditory sensation. Then the visual imagery dropped out, meaning that I could get no aid in the problem from the imagery" (H).

4. It may remain or recur (a) as a whole, (b) in part, (c) changed in form or meaning or both, to serve as all or part of the anchoring imagery of the problem. Number of instances=50.

Stimulus: red—fragrant—alive. Report: "'Red' and 'fragrant' called up a visual image of a red rose on a small bush. Then in verbal-motor imagery, 'alive, alive; that's alive.' This imagery remained until E set the second situation (Problem: A man wanted to catch a kitten but the kitten ran up a tall tree which no person could climb. How could he get the kitten without hurting it?). The reading of the problem was accompanied by a shift in imagery. The thorns on the tree became large and looked wicked, meaning 'No person can climb it.' They also meant a very large bush like a tree. The rose was still on top and the kitten was near the rose" (Z).

5. It may remain or recur (a) as a whole, (b) in part, (c) changed in form or in meaning or both, to serve as the material for solving the problem. Number of instances=31.

Stimulus: prison—stripes—chains. Report: "The 3-word imagery was quite good and was of a prisoner in stripes with ball and chains on his leg. Then came the problem (Problem: How do you play 'Snap the Whip?'). There was verbal-motor repetition of the problem which helped me to remember it. The visual imagery was modified and now included a lake and skaters. The prisoner stood still in the middle of the lake as if the place where he had been in the 3-word imagery was now the ice. The 'ball' became a man and the 'chain' a string of skaters. Then the string of skaters moved and the prisoner stood there" (D).

6. It may drop out as soon as the problem comes, but may recur after the answer to the problem has been given, in which case it may



be (a) illustrative of the problem, (b) non-illustrative of the problem. Number of instances=6.

Stimulus: cat—yarn—basket. Report: "The 3-word imagery came. Then the problem was read (Problem: Suppose that you have been sitting in one position for a long time and your foot goes to sleep. What is the thing to do?). I hardly know what happened in the solution. I had a visual image which meant my foot going to sleep. This was an irregular patch with bright zigzag lines. Then I said the answer and back came the 3-word imagery" (D).

7. It may serve as a background into which is projected a person or the *O* himself who solves the problem. The background may last throughout the problem-solving or drop out before the problem has been solved. Number of instances=12.

Stimulus: dew—garden—silence. Report: "A visual image of a nice flower garden in the early morning before sun-up. I was in it. When the question came that visual image was still there vividly and I was solving the problem, projected to that garden (Problem: Why is an octave so named?). I was in the garden thinking: 'octave, octagon' in verbal-motor imagery. Then came the verbal-motor 'eight' and I said, 'do, re, mi, fa,' and gave my answer. The 3-word imagery was there until the end, but I was the important part in it" (Z).

8. It may recur to serve as a means of escape from solving the problem. Number of instances=2.

Stimulus: doctor—lawyer—minister. Report: "The 3-word imagery was present sharply and the problem came and was carried partly by visual imagery which was in the lower right part of the field (Problem: If 3 oranges cost one fifth of a dollar, how much will 2 cost?). The rest of the problem was carried by verbal-motor imagery with here and there a visual tag, but the solution seemed to be in terms of verbal-motor imagery. The 3-word imagery recurred at least 3 or 4 times. It seemed to be impossible to solve the problem. Later I made a specific effort. Once I said: 'Report that you can't do it.' Then by forcing myself I got the answer. Near the end there was a recurrence of the 3-word imagery. It was very, very scrappy" (D).

9. It may persist either through part of the solution or until the end of the solution of the problem, in which case the problem-imagery is (a) in front of it, (b) at its left or right. Number of instances=10.

Stimulus: infant—manger—wise men. Report: "There was good 3-word imagery. It stayed, so that there was no break when the problem came (Problem: How was Achilles made invulnerable?). When 'Achilles' was mentioned he seemed to be at the left of the other image; larger and clearer. He was running. Then I started to give the answer" (S).

10. It may remain throughout the solution until the answer has been given, in which case the *O* (a) can give no report, or at least no certain report, of the experiences leading to the answer, (b) states that the answer came immediately or automatically. Number of instances=5.

Stimulus: chimes—snow—carols. Report: "The 3-word imagery was very good. Then came the reading of the problem and its meaning was carried in verbal-motor imagery (Problem: Where do corks for bottles come from?). The 3-word imagery was still there and almost as good in detail as before. There may have been a little

tag of visual imagery which carried part of the meaning for my answer; I am not sure. About here I found myself answering. I didn't know I was going to answer until I heard myself" (D).

We are already familiar with some of the foregoing headings, so that a detailed discussion of them will not be necessary. The cases in which the 3-word imagery dropped out before the problem came included those in which the words had not evoked any imagery at all, and those in which the imagery was poor in detail and in clearness. Frequently, too, the set to solve the problem caused a clean break when the problem came, although the 3-word imagery might have been good. The cases in which the 3-word imagery remains or recurs during the reading of the problem or its solution are illustrative of the conflict between the *Aufgabe* and the 3-word imagery. The determination to solve the problem may overcome the 3-word imagery before the solution is actually entered upon or, as in the other possibility mentioned, the alternation between the problem-imagery and the 3-word imagery may continue during the solution of the problem. We find, too, that there may be a recurrence of the 3-word imagery after the answer has been given, that is, after the determination to solve the problem has been satisfied and the *O* is free. These cases are evidences that we succeeded in giving our *O*'s a background of imagery. A suggestion as to why the 3-word imagery dropped out is found in the report of an *O* who said: "The visual imagery carrying the 3-word situation came back very briefly during the reading; then it dropped out, meaning that I could get no aid in the problem from the imagery" (H). This brings us to the cases in which the *O* did secure aid from the 3-word imagery. To just what extent the imagery was used in the anchoring and the solution of the problem the illustrations will have shown. That this is the economical mode of behavior for the 3-word imagery is obvious. The *O*'s had a background of imagery, and upon this they drew in their solving of the problems. Frequently, only that part of the 3-word imagery which was relevant remained, the irrelevant part being lost; a further proof of our thesis that irrelevant imagery does not exist. We furnished our *O*'s with imagery logically irrelevant to a situation in which they might need imagery. If no aid could be got from the 3-word imagery or, in other words, if it was totally irrelevant, it dropped out. If, however, it could be used, though only in part, it so far remained. The cases in which the imagery became changed to carry the meaning are of interest. They show again the tendency to economy and, as well, the importance of relevancy. The cases in which

the imagery serves as an escape from solving the problem are like those met with in Experiment A. The cases in which the 3-word imagery remains as a background during the solution of the problem are further illustrative of use pushed to its limit. The *O* has been revelling in a field of imagery from which he need not wholly withdraw; that is, the 3-word imagery remains relevant to the new situation.

Before we consider the cases included under the two last headings (9 and 10) it seems well to discuss the attitude of the *O*'s toward the instructions during the last part of Experiment D. The smallest number of observations made by any one *O* was 55, the largest number, 73. The same instructions were used throughout the experiment and the same kind of material. For the sake of convenience and somewhat as a matter of course, the material was divided into 4 groups; the first group containing 10; the second, 13; the third, 15; and the fourth, 35 three-word situations. So far as the *O*'s were concerned, the instructions underwent a process of specialization. That is to say, the reported behavior of the 3-word imagery at the beginning of the experiment is quite different from its behavior at the end. Let us, for purposes of discussion, divide our classification of the behavior of the 3-word imagery into 5 sections. The first section will include those cases in which the 3-word imagery dropped out either before or at the very beginning of the reading of the problem. The second section will include all cases in which the 3-word imagery hung over during either the reading or the solution of the problem, but in which it did not aid in the anchoring or the solution of the problem. In the third section will be placed the cases in which the 3-word imagery was used as material for anchoring or solving the problem; and in the fourth section, the special and relatively infrequent uses of the 3-word imagery listed under headings 6, 7, and 8 of our classification. There remain for the fifth section the cases in which the 3-word imagery was present throughout the entire solution of the problem. All 4 groups of the 3-word situations will be found in the first section. This is to be expected, since the adequacy of the 3-words for setting up imagery cannot be predetermined, and such a word-situation may occur in any one of the 4 groups. That the *O*'s were, as time went on, more exactly obeying the instructions to take a passive attitude is shown by the relatively small number of cases in group 4 in which the 3-word imagery dropped out before the reading of the problem. In the cases included in the second section it is clear that the 3-word imagery is of more importance than it had been before. The *O* does not succeed in getting rid of it when the problem comes. This is perhaps due to the assumption of a more passive attitude with respect to the 3-word imagery. Nevertheless, the state of affairs is not satisfactory to the *O*; for he is hindered in both his comprehension and solution of the problem by the 3-word imagery. Group 4 of the 3-word situations contains the smallest number of cases of this kind, and group 3 the next smallest. This means that as the experiment went on the *O* was making a better adaptation to the complicated situation. When we come to a consideration of the third section we see at once a change. The 3-word imagery does not drop out with the coming of the problem, nor does it remain to disturb the *O* in his solution of the problem. It is used. The *O* has succeeded in meeting the situation with a minimum of



annoyance to himself. Two *O*'s did not reach this stage until the fourth group of 3-word situations, and one *O* never reached it at all.

The cases included in the fourth section we have discussed above. They are all illustrative of the uses of the 3-word imagery and, for the most part, occur in groups 3 and 4. What has been happening has been a specialisation of the instructions which is correlated with a successful adjustment to the situation. The specialisation has been in favor of the 3-word imagery. Instead of being a factor to be eliminated, it becomes one of value in meeting the situation, that is, in solving the problem.

In the fifth section, which includes headings 9 and 10 of our classification, the specialisation is continued.

In stages 9 and 10 the 3-word imagery becomes so favored that it remains through the solution of the problem, either together with the problem-imagery or to the exclusion of the problem-imagery. There occurs a shift of relevancy from the problem-solving to the experiment as a whole. What the *O* does is to attend to both kinds of imagery, the 3-word and the problem. He is interested now, not so much in solving the problem, as in giving a good report; a report that shall describe both the 3-word and the problem imagery. This sort of reaction to the experiment is confined to 2 *O*'s, and occurs in the case of one of them altogether within group 4 of the 3-word situations and in the case of the other within groups 2, 3, and 4. The reports show that in these instances a fluctuation between the 3-word imagery and the problem-imagery takes place, and that in the fluctuation the 3-word imagery does not completely disappear, though it loses in clearness and sometimes disappears for good before the problem-imagery disappears and the problem has been solved. The 3 cases of superposition of the problem-imagery on the picture-imagery referred to in part 1 of Experiment D,<sup>39</sup> and the one case in which the imagery used in the completion-test remained during the solution of the problem noted in part 2 of the same experiment,<sup>40</sup> are similar to the cases described here. They were reported by the same *O*, a fact which is further evidence of a special interpretation of instructions and of a shift in relevancy from the problem solving to the experiment as a whole.

In the 5 cases (confined to the reports of 2 *O*'s) found in part 3 of Experiment D and included under the tenth heading of the classification of the behavior of the 3-word imagery, the 3-word imagery is altogether favored. The *O* can give no report of the processes leading to the solution of the problem. In some instances he states that there was some pro-

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<sup>39</sup> Cf. p. 222 above.

<sup>40</sup> Cf. p. 223 above.

cess present but that "none came into great clearness." The other *O* stated that the answer was given automatically. In this connection we would call attention to the fact that in none of the 5 cases did the *O*'s have any doubt as to the relevancy of the imagery present. In every case they were certain that the imagery belonged to the 3-word situation and not to the problem, though they could not report on the imagery used in solving the problem. If, as Dr. Koffka thinks, we do not know whether or not our imagery in a given case is relevant or irrelevant, the *O*'s might well have reported, since they were specifically asked to give such a report, the 3-word imagery as the carrier of the solution of the problem. But this is exactly what they did not do.

The results of this experiment (D) confirm the results of those preceding. We have shown that, when imagery is occupying the focus of attention, if a new situation bearing no relation to it is introduced, two things may happen; the imagery either drops out, or it remains and is used in the new situation. We have found also that there may occur a specialisation of instructions, which means a shift of relevancy from one attitude to another, but that in no case is there any irrelevant imagery reported.

#### GENERAL CONCLUSION

All of the experimental work has led us to one conclusion, that *there is no irrelevant imagery*. That at the outset we had expected to find it is clear from our experimental procedure. We hoped at first to get some evidences of it indirectly; and when we did not succeed we arranged situations in which there were irrelevant factors. The analysis of irrelevanties on the perceptive level showed that they are principally characterised by a feeling-component. This feeling-component we found in later experiments, though it was never a characteristic of irrelevant imagery, but always of a total situation irrelevant to another situation, each of which situations had its own relevant imagery. When we finally succeeded, in Experiment D, in giving our *O*'s a background of imagery logically irrelevant to a situation later introduced, we still found no irrelevant imagery. In such a case, if the imagery could be used either as a whole or in part, it remained; otherwise it dropped out. We can, then, answer Dr. Koffka's criticism that "sensory contents may be irrelevant to the thought;"<sup>41</sup> for we have seen that, if imagery is present, it is relevant; and accordingly, if the author to whom he

<sup>41</sup> Koffka, *op. cit.*, 219.



refers meant by analysis "nothing else than the exhibition of the sensory contents present at any given moment,"<sup>42</sup> she was right in assuming that these contents were relevant to the thought.

We have called attention to the fact that not only did all of the *O*'s use imagery but that they frequently expressed a felt need for it. We have also brought evidence that this dependence upon imagery is not the result of laboratory conditions.

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<sup>42</sup> *Ibid.*, 219.

## AN OBJECTIVE INTERPRETATION OF MEANINGS

By J. R. KANTOR

Paradoxical as it may seem, it is still true that the problem of meanings, which provided so much difficulty for the introspective psychologists, meets with a comparatively simple solution by the methods and materials of the objective psychologist. The introspective psychologist experienced great difficulty in the interpretation of meanings because, presumably, the latter were supposed to possess essentially an inner character, which could not be identified with mental content of any particular sort, meanings being considered still more central than sensations or images. Nor is this difficulty much minimized by the parallelistic behaviorists<sup>1</sup> who translate the specific meaning-factor into partial movements of the eye, hand, or some muscle. For a meaning is in no sense a thing or a content either mental or physiological, but rather an act or an adjustment of the person,<sup>2</sup> which conditions another and following reaction.

### I

Our first and fundamental departure from the traditional descriptions of meaning-functions may be summed up in the statement that meanings, in common with all other data of psychology, are definite responses to stimulating objects and conditions. By meanings we understand specific characteristic differential responses to particular stimulating objects in their appropriate settings. Now as meaning-reactions these responses differ from other differential reactions in the fact that the former are not complete adaptations or final adjustments to stimulating circumstances, but rather their function is to condition the specific operation of another succeeding or consummatory reaction. Thus my perception, that is, the appreciation of the presence or the identity of the book that lies before me on the table, is a precurrent or anticipatory response conditioning the further act of picking up the book or allowing it to lie undisturbed.

<sup>1</sup> From our standpoint, of course, a psychological theory does not lose its parallelistic character even though the adherent thereof rejects one or the other series of supposedly parallel phenomena.

<sup>2</sup> That is to say, the complete operation of a reaction system.

Let it be at once noted that a meaning-reaction differs from any other kind of psychological response only in the fact that it serves to condition a succeeding act. Otherwise, it may comprise the same number and kind of factors, such as cortical centers, neural pathways, affective components, etc. In other words, meaning-reactions differ from other reactions only in function. In consequence, they represent the acquisition of various combinations of reactions, such that when the stimulus calls out one of them, it will be a means of bringing the other or others into operation. In other words, meaning-reactions consist of specific integrations of precurrent and consummatory responses, the former of which are already linked with specific stimulating objects or conditions. Thus, when the signal light flashes into the visual field of the locomotive engineer, the perceptual response is coupled with a series of other responses which result in the stopping of the locomotive. From this stand-point the meaning-reaction derives its name from the fact that it serves as a definite means for the functioning of some given reaction, picking up the book or pulling back the locomotive lever.

Since the unit of behavior or psychological reaction is the segment of behavior, or the response to a stimulus, we can, by referring to such a segment of behavior, indicate more precisely how the meaning-reaction operates. In detail, this operation is as follows. The stimulus-object brings into function a definite reaction-system which has been developed in direct contact with the object in question.<sup>3</sup> When so developed, this reaction-system operates as an evaluatory response in the sense that it serves to mediate an appropriate final adaptation to our illustrative object.<sup>4</sup> The point here is that, when we develop a differential reaction to an object in a given setting, we have appraised and evaluated the object from the standpoint of our behavior toward it.<sup>5</sup> This point is illustrated by the fact that all but the most abstruse definitions of things are stateable in terms of how we react to things. A table is "to put something on," as the child describes it. These evaluatory responses are developed, of course, with reference

<sup>3</sup> By a reaction-system we mean the series of factors; sensitivity to stimulus, receptor and effector mechanisms, neural activity, muscular and glandular functioning, etc., which make up part of a psychological response.

<sup>4</sup> A segment of behavior consists of a stimulus and the series of reaction-systems (when there is a series) that is the pattern of response, which constitutes an adjustment to the stimulus.

<sup>5</sup> This development takes place irrespective entirely of whether the person knows it is taking place.

not only to objects but also to the specific qualities and relations of objects, as well as to events and conditions of every description.

When a quality of an object is evaluated, we must observe that in many cases the person's reaction may be much more passive, or we might say the object is evaluated more on the basis of what it does to the person than upon what the person does to the object. In such cases the evaluations of things and their qualities go back to a non-observable condition or action in the organism, possibly an electro-chemical change, which we refer to as the differential sensitivity of the organism to color, taste, temperature, etc. Although, such activities of the organism are hidden from the view of the observer, whether it be the reacting person or some other, they are none the less definite acts, precisely as are the digestive acts, the occurrence of which is unseen and may even be unknown to the food-consuming person. It is possibly not an imprudent suggestion that, while the substantive meanings of simple perceptual objects are derived from active operations upon such objects, the adjectival meanings, on the other hand, are derived from the more subtle and for the most part unobservable reactions which we call sensitivity. In passing we might suggest that our term sensitivity is used in the sense that the physicist uses the term when speaking of the deflective actions of a galvanometer. Obviously, whenever any meaning-reaction is named or brought to the notice of oneself or others, a positive observable reaction is being performed.

Impossible it is to omit in a discussion of meaning-responses the specific reactional auspices under which the meaning-acts are performed, since the stimulating circumstances are not by far the least important of the conditions for the origin and operation of meaning-responses. This means to say that the meaning or significance of an adjustment is entirely a function of its appropriateness in any given circumstance. In other words, meaning-functions symbolize specific forms of interaction between objects and the person's responses to them. The kind, number, and relations of objects to which we react force us to develop combinations of responses for adaptational purposes. What an object means is intrinsically the problem of the particular type of response which the person has developed while in contact with it. As a psychological term, therefore, meaning refers (1) to the significance which an object has for the person as indicated by his reaction to it, and (2) to the particular type of reaction which a given object brings about in the person.

From our standpoint there is nothing strange or inscrutable about meanings. The fact is that the meaning-reactions which appear as such effective adaptational aids to the person are merely the anticipatory responses which the various surrounding stimuli have caused persons to build up or otherwise to acquire. A meaning-reaction is accordingly a fitting adjustmental response which individuals acquire through the direct influence of the surrounding objects and conditions. It is in this way that a meaning-reaction becomes the means for bringing about an especially fitting adjustment of the person to his surroundings. And it is this type of precurrent meaning-reaction which permits us to say that a person foresees the consequences of an act. For it is precisely such meaning-reactions as we have described which make it possible for us to have any delayed reactions. We might even go a step further and say that it is the development and operation of these anticipatory meaning-reactions which are celebrated by the term consciousness. We may repeat, then, that a meaning is a psychological action, in no sense distinct from the many other definite responses which we are hourly performing. In other words, a psychological meaning is not anything mental or psychic, nor is it merely a peculiar muscular or glandular reaction paralleling a mental state, but rather a meaning-reaction is *any* reaction of the person which stands for or signifies a thing or condition by causing a particular response to it.<sup>6</sup> But of course meaning-responses differ from each other and from other members of a segment of behavior in precisely the same way as any two human reactions may differ.

## II

Up to this point, namely, as long as we are discussing direct responses to present stimuli, everything seems clear and definite enough. But uncertainties appear at once when we consider the behavior in which the objects to which we adjust ourselves are not immediately present. How, we might ask, can we interpret the meanings and intentions concerning persons, things or other stimuli which are not at the moment within range of our actions? Here we have the problem of the detached meaning-reactions which are usually referred to as thought and imagery. To the objective psychologist it

<sup>6</sup> Regardless, of course, of whether the person knows what is taking place. In order that the reacting person should also *know* what is going on, he must not only be determined, because of some precurrent reaction, to perform a definite final adjustment, but in addition he must be able to report to himself verbally or otherwise that such an event has taken place.



seems an extreme error to overlook the unquestioned continuity between what may be called the perceptual and non-perceptual meaning-reactions. Imagery, or non-perceptual, meaning-reactions are no less definite responses of the person than are the meaning-reactions contained in segments of behavior in which the original object is present.

As a matter of fact, every genuine perceptual response already involves a partially detached or implicit reaction, in the sense that the specific perceptual phase of the segment of behavior is a revival of a differential response to qualities and conditions of objects not at the moment in actual contact with the organism.<sup>7</sup> The main point here is that a differential reaction system or pattern of response which was acquired in the original contact with the object in question is upon a second or later contact with the same object put into operation in the same way or in a slightly modified form.<sup>8</sup> When the object which originally caused the reaction to be built up is absent, the differential reaction system can still be put into action by a substitution object. In this case, of course, the reaction system will be an implicit or an incipient operation. The response will occur only in part or in some lesser degree. There is no reason to suppose, however, that such an implicit response to an object not present, does not involve the same neural, receptor, and effector apparatus.

Furthermore, since all implicitly operating reaction systems, no matter how closely they resemble the original act, are substitutive reactions we are not surprised that it is possible for them to become symbolic. That an implicit reaction system determining a succeeding response can be very unlike the overt act which would ordinarily stimulate the succeeding response, or entirely different from the overt response which a given object elicits, is traceable directly to the fact that every implicit reaction is already in some sense a representative activity. Whenever we respond to an object not present, be it in a dream, revery, thinking, or planning of whatever sort, then we are symbolizing or representing the object or situation constituting the original stimulus to our behavior. Now although there is no limit to the degree with which the implicit response varies from the original reaction to the same object, still there are definite determining conditions which make for the specific symbolization of objects by particular

<sup>7</sup> Cf. Kantor, *Suggestions Toward a Scientific Interpretation of Perception*, *Psychol. Rev.*, 27, 191 ff.

<sup>8</sup> Such modification depends upon the setting, or the total stimulating situation.

reaction systems. These determining conditions are for the most part contained in the original environmental setting of the object which is being reacted to. Just why my implicit reactions to the city of Washington should invariably involve a symbol of fatigue is no doubt explained by the fact that my first contact with that city included an enervating round of continuous sightseeing. As every one knows the implicit reaction systems of thinking processes appear in most cases as entirely unlike anything that we should expect to be connected with the original stimulating situation. No further illustration of the peculiarities of symbolized implicit behavior is necessary than a reference to the facts of individual imagery differences.

Although it may not at all reinforce our conviction that an implicit meaning-reaction is a detached response to an object, it is well to observe just how a reaction system or response pattern can be detached from a situation in which it was first acquired. We have here really two problems. Not only must we account for the detaching of reaction systems, but we must also describe the mechanism by which such detached reaction systems are put into operation by some substitute for the original stimulating circumstance. Naturally enough these are reciprocal problems and the solutions are closely intertwined.

In general, the possibility of detaching reactions from their original settings goes back to the elementary fact that human persons are from the psychological standpoint organizations of response systems. A psychological fact consists of the operation of one of these reaction systems resulting in the adjustment of the person to an object or condition. Of the most elementary of these adjustments are the connate reflexes and the sensitivity to colors, sounds, etc. By contact with objects these primitive responses may become integrated into more complex adjustments to objects or into reaction patterns; so that the person will respond not only to color and shape, but also to the complex objects as a whole. Nor does the reaction to objects define the limit to human reactions. Suffice it to suggest that the next hypothetical step is the organization of the reaction to include the setting of the object. Mark well our point, namely, that the whole of the individual, psychologically speaking, is an enormously expanded series of such potential responses.

Now it so happens that such reactions are sometimes called out when the original objects responsible for their existence are absent. That this operation of the reaction system can

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occur without the instrumentality of the original stimulus is accounted for by the existence of some common factor in the present and past situation. And so the infant may proceed to suck any object, although it may only very remotely resemble a nipple. In more complex situations the common element responsible for a given reaction may be the setting of the stimulus, and not any quality of it. Illustrative of the latter situation are the mistaken reactions resulting when we respond to a stranger in a way in which we have accustomed ourselves to react to a friend, who usually is found in the place which the stranger now occupies.

Again, a person is stimulated by wants, organic necessities, and desires, to reinstate some reaction system previously built up under entirely other auspices. A hunger reaction may stimulate us to reach for and perform other incipient reactions to food-objects, although no food-objects are to be found in the vicinity. To a certain extent, also, we might consider that the process of detaching reactions from the original stimulating circumstances in which they occur is merely a process of continuing an action once the original stimulus for it has passed. As an example we might refer to the tense organic strain and reverberation of the person who virtually is unable to stop running or rowing for some time after the actual contest is over.

We may conclude, then, that the different mechanisms for detaching reactions from their original settings involve in some sense a rearousal or a functional continuance of a reaction system. When the rearousal occurs we may trace the stimulus back (1) to some object or condition other than, but previously connected with, the original stimulus object, or (2) to some present reaction of the person serving as a stimulus to such reoperation of a reaction system. No matter how the rearousal takes place, the detached reaction may become a means and a determiner of the operation of an associated succeeding response. What this associated response is depends upon the specific experiences of the person. Plainly, it is extremely important for the process of detaching reactions from their original stimuli-response situations that there be some similarity and resemblance between the various objects and situations concerned; so that they can substitute for each other as stimuli.

### III

Thus our discussion brings us up squarely against the problem of the image, since it is pretty clear that imagery reactions are detached vestiges of sometime overt reactions to con-

crete objects. Imagery responses, therefore, must be considered as one of the types of the individual's reactions, along with overt responses and others which determine the immediately succeeding reactions to things. What the proposed hypothesis demands of us to believe is that every psychological process is a specific operation of the person to given objects or situations. To accept this theory means to believe that any perceptual activity is a particularized action, performed when stimulated by any given object with all its qualities, as it comes into direct contact with the person. That is to say, the theory demands that we do not assume that the qualities of things or the things themselves exist as central material, or *in consciousness*, as the textbooks put it. Lest there be any question as to our meaning at this stage of our discussion, we reiterate that, in every case of psychological reaction, perceptual responses by no means excepted, the person reacts to a thing in which inhere all of its qualities. Hence we are not obliged in the case of imagery reactions to account for central qualities. The difference between perceiving and imagining a book lies in the fact that in perceiving it we react to it with its qualities, while, when we imagine a book, we must supply the qualities and relations of things, at least when the imagery is vivid, by means of verbal or language substitutions. This fact accounts for the usually greater vividness of the perceptual reactions. And here we might suggest that the traditional difficulties with imagery reactions are born of the prejudice that the image which is presumed to be the carrier of non-perceptual meanings is a peculiar central process absolutely independent of receptor systems and muscular mechanisms, in addition to being otherwise related than are sensory processes to cortical centers. From our standpoint an image response is a reaction system, in principle precisely like any other, involving exactly the same factors, but differing from other reaction systems in that the former are in some manner distantly removed from the primary stimulus-response situation in which they originally developed.

And what precisely is an image? Why simply this, a vividly repeated reaction system or pattern of response to some specific situation, plus the speech reactions descriptive of the objects and events reacted to. In many cases the nature of the image depends upon its verbal purveyance, in the sense that the reported qualities of imaged things are supplied by the person himself. This fact is clearly demonstrated when we attempt to imagine an object which we have



looked at some time ago or perhaps have never actually seen. The infidelity of testimony illuminates the possibilities in the way of adding materials to past objects and events. To be entirely specific at this point, we must indicate that much of the image experience is really a verbal self-analysis of how we respond to an absent object. Thus, contrary to popular conception, the most intense imagery reactions are those in which the person repeats verbally the scene he has just witnessed or has otherwise partaken of. Consider the young lawyer just returned from his first important legal argument, or the lover rehearsing before himself as auditor the scene just preceding the capitulation of the most adored one. The amount and intensity of the imagery depends upon the impressiveness and the intensity of the original situation.

Although much of the image reaction is verbally supplied and verbally stimulated, yet the basic fact in any imagery experience is the actual responsiveness and organic functioning which is involved in the implicit repetition of a person's activity. Anyone attempting to recall what happens when deeply suffering from the pangs of remorse, or writhing under the lash of insult or the sting of disappointing dejection, will have no trouble in appreciating the definite activity of the person. Impossible it is to overlook the shuddering of the entire person, the palpitation of the heart, the activity of the salivary, lachrymal and other glandular processes which take place, when we remember or think about some distressing or very pleasant experience. Above all, we must bear in mind always that we are attempting to describe the behavior of a tense superactive organism, and not the products of a cold logical analysis. What happens when any of our imagery experiences become blunted, as is practically always the case with the passing of time, is that the pulsation and quavering of the organism have subsided.

From the fact that the core of imagery reactions is a definite liberation of the person it follows that by far the most vivid imagery is that in which the individual is himself living over a profoundly impressive experience. So vivid at times is such imagery that one may appear to be in a certain place and actually to speak and otherwise react to persons, when as a matter of fact one is at great distance from both the person and the place which are now being implicitly reacted to. Moreover, an implicit reaction to a past experience may involve almost as much actual expenditure of energy as an overt adjustment to a less intense situation. Less vivid, naturally, are the imagery responses which constitute



implicit reactions to conditions with which one is not personally concerned. In general the energy with which our implicit reactions operate depends upon our capacity to relive the original situation. And so the warmth with which one sympathizes with another person who suffers some tragic experience is dependent upon the fact whether the sympathizer has himself played a part in a similar event, and is consequently able to relive it, to image it better. Here we find the psychological basis for the emphasis which the employer places upon *experience* as a qualification for employment. It is this, that having previously made reactions to a type of situation, one is now better equipped to react implicitly to the same or a similar situation and thus be more resourceful in the present circumstances.\*

Because the implicit meaning-reactions are so easily performed and occur so much more readily and quickly than the explicit type of response, and moreover because the former are so subtle and representative, they serve as the most capable and efficient of meaning-reactions. Indeed, even though the older psychologists did not fully recognize the character of imagery, they hit upon images as the exclusive type of meaning-responses. Not even the mistake of making images the only type of meaning-reactions can rob those psychologists of the credit for their insight into the character of imagery reactions. Just how efficient these implicit reactions are may be gathered from the consideration of a thought or a planning segment of behavior, in which the compelling stimulating circumstances induce a very intricate interplay of implicit reactions, serving on the whole as backward references to events in the life of the person important to him in the particular circumstances, besides enabling him to anticipate future possibilities of action. Of course in any serious problematic situation the person will combine such implicit responses as we have attempted to describe, with explicit handling of maps, statistical tables, drawings, slide rule, books, and other such instruments of complex human behavior. The explicit acts serve as stimuli to actions as well as being themselves adaptive responses. We hasten to add that, by considering the actual complex of implicit and overt reactions constituting a planning behavior, we gain insight into the actual continuity of such actions throughout all their variations.

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\* We can no better illustrate the repetitive nature of an implicit reaction than by pointing out the necessity of repeating reactions in order to recall or understand them, or to reproduce facial expressions in order to appreciate their significance or to name them.

## IV

If our hypothesis concerning the nature of meaning-reactions is valid, it follows as a matter of course that the implicit meanings cover a wide range of precurrent responses. Now as a matter of fact, all of these responses may be roughly grouped between two limiting classes, which we will name representative and substitutive. By a representative meaning-reaction we understand a reaction system similar to one performed upon a previous occasion and now serving as a determiner of a succeeding reaction. Thus the representative meaning-reaction is a direct vestige of a previous reaction situation, and consequently is morphologically a fairly overt response, in the sense that it incipiently repeats a former response. Such representative reaction systems, therefore, stand upon the borderline between the overt and implicit reactions. We may consider as examples of the representative meaning-reactions all of the imagery responses that we have discussed; remembering, however, that other representative meaning-reactions involve the elaborate movement of external skeletal muscles.

At the opposite pole from these representative reactions stand the substitutive responses, which, though functioning as determiners of reactions, are themselves morphologically completely at variance with the overt act producing the same result. These substitutive responses, while of course definite acts of the person, do not in any sense resemble the reactions to the objects for which they substitute; they may be totally symbolic; and so difficult is it to seize hold of such meaning-responses that their operation in many cases is frankly inferred. The fact is that the symbolic reaction may involve such an act on the part of the person as he himself has no notion of. In the literature of psychology the substitutive or symbolic reactions are denominated concepts or thoughts. So distantly removed are the substitutive responses from the original conditions to which the person is adjusting himself, that the stimulus to thought action is a total situation or problem. Now, the detailed facts of the stimulating situation are supplied to the person by drawings, writings, or verbal stimuli. The general problem, however, may be looked upon as the directing and controlling stimulus.

Between the fully representative and the substitutive reactions we find interpolated the language responses which constitute a most efficient form of meaning-reaction. The marvellous effectiveness of the verbal reactions to determine behavior lies in the fact that they are not only completely

overt morphologically, but they represent the most facile of all our performed actions and at the same time they are capable of infinite modification. In consequence, verbal responses are among the most satisfactory substitutes for all sorts of objects and acts. Finally, the language reactions constitute so pervasive a form of human activity, that they connect with and bring to the surface the deep seated conceptual responses. Thus, the conceptual responses, for example, are most serviceable for reactional purposes when they are associated with language acts. This fact is evident when we consider that ideas are simple concepts which, because of their attachment to verbal reactions, serve to induce responses in oneself and others.

## V

Before proceeding to a brief description of the conceptual reaction we might raise the question how the implicit reaction, which resembles in no way the original object or the original reaction, can be said to be a reaction to that object at all. The answer to this question is found in the consideration of the manner in which the reaction operates. The substitute reactions operate as precurrent or anticipatory responses to some other final reaction, and this is exactly why they are meaning-reactions. As we have already indicated, the entire significance of a meaning-reaction lies in the fact of its operation as a determiner of a succeeding final response to a given stimulating object or condition. Whether a given reaction system is a response to a particular stimulus depends entirely upon its functional connection with that stimulus.

Concepts are reaction systems which operate when it is necessary for us to make immediate use of large segments of our past experiences in rapid and effective ways. The mechanism for this activity is as follows. Some problem presents itself to us, the construction of a bridge, let us say. It is necessary for us to correlate this problem immediately with other problems of a similar sort, in order to make plans for the new structure or to draw up an estimate of cost. In terms of the old work, old conditions of all sorts, we must project and create the new object or condition. For this purpose we have a stock of concepts or ideas representing our past experience of a particular sort, which now conditions the actions of making plans, submitting estimates, and directing the actual bridge construction.

The concept is therefore a vestigial remnant from a previous situation or rather a series of situations; for a concept

is a standardized and definite implicit response which substitutes for and sums up the person's experiences in a form useful for present purposes. Plainly, the capacity to develop concepts depends upon verbal aids, since only through such symbolic means can we build up such meaning-functions as concepts are. Not only are language functions necessary for the development of concepts or standardized meaning-reactions, but the use of them depends very closely upon word responses. It is through words (spoken, written or printed) that concepts are primarily stimulated to action. Hence we may look upon a treatise as an extension of the person's implicit reactions to things. By means of a treatise we are enabled to sum up and record the significant facts of our past experience.

Incidentally, there is brought to our attention the distinction between different types of concepts, namely, those standard implicit reactions summing up our own experience, and those concepts which are derived from our indirect contact with things, from our reading and hearing of bridges built, for instance. Obviously, the largest number, by far, of our more important concepts are derived in the indirect way, although it is an essential factor in the development of a concept that the person must have had some personal experience with the fact or conditions represented. The degree to which we are unable to grasp a concept or have one communicated to us is a direct function of our lack of experience with the facts and conditions represented. When we have had no actual contact with a certain object or condition we can have no concept of it, and therefore we can only have verbal substitutes. Every teacher, it is almost safe to say, has met with persons who function as students entirely on the verbal level, or almost so.

In considering the differences between a scientific concept and one developed in everyday life we cannot overlook the deliberate operations which the scientist performs upon the materials with which he deals. We might look upon this deliberative activity as in part a direct manipulation of materials and apparatus and in part an implicit handling of these materials with direct reference to previous experiences of the same sort. All of this activity is performed with the constant aid, and by means of, communicative language activity, both stimulative and responsive.

The distinction between scientific and everyday concepts marks a difference in the levels of our behavior. These levels may be arranged in series from the mere implicit contact of



the person with things to a thorough understanding and manipulation of objects for certain definitely appreciated purposes. The actual basis for the distributive arrangement of the levels of behavior lies in the intimacy of the contacts of the person with the objects and conditions to which he is adapting himself. Clearly, the intimacy with which one is in contact with surrounding things is not in direct correlation with the overtness of the response. For obviously we may produce more important effects upon things by the indirect responses of thinking than by most direct contacts of a perceptual sort. Now in point of fact, the scientific concepts operate in a more remote way upon objects than do the everyday concepts. As we have previously suggested, we may consider scientific concepts to be reactions operating upon a level of deliberated and motivated action and therefore very different in degree from everyday concepts. From the fact that scientific concepts are practically identical with ideas, we may infer that the difference in degree between concepts parallels such crystallization of reactions as to make them available as stimuli to actions.

In a sense, a conceptual act is a self-stimulating reaction to further implicit behavior, so that a thinking activity which is essentially a manipulation of concepts is a continuous activity of the person with respect to some object or condition. When the conceptual reaction becomes so standardized and identified with a term or name that it may serve as a common stimulus to various persons, then we may call it an idea. The intimate connection between the concept and the communicative language form makes the concept a definite object of scientific technique, much as a piece of apparatus is. Absolutely essential is it for the functioning of ideas that they be embodied in language forms (names, etc.) and it is true, as a matter of fact, that such ideas are indistinguishable from the expressive language which serves as their medium of circulation. Thus language appears as an indispensable tool for both the operation and the expression of thinking.

## VI

The development and functioning of language responses indicate most excellently the facts of meaning-reactions. For language is essentially a determiner of action, whether in the vocalizing person or in some other. In the fact that action is determined in others we find a basis of division of language mechanisms into mere vocalization and communicative



speech. The latter type of meaning-reactions are not, however, confined to communication with others, for obviously one may also communicate with oneself. The differentiation between verbalization and true language activity, or between mere speech and communication, involves a difference in the total behavior of the person at the time. Essentially the total situation includes not only the responses of the person but the stimulating circumstances also. As to the latter, they must be such as to interest or challenge the reacting person, interest him in the form of satisfying his wants, as the mother or nurse functions to the desiring child. Or the stimulus may arouse or awake the person to a response which will in turn induce in the stimulator another response. Here we have of course ordinary conversation. As contrasted with mere verbal utterance as a determiner of behavior, communicative speech involves a definite attention set and an attitude of serious expectancy which puts the person in close *rapprochement* with the stimulating person.

All language in the sense of action determiner or verbal stimulus goes back of course to the articulate function of human beings, and we might assume that speaking or the use of words, both as mechanism and as psychological acts, derives directly from crying through the path of vocalization. All the observable facts of human language are constituted by the processes in which the crying mechanism and action develop to the stage of definite communication.

Much light may be obtained concerning the development and use of language from a study of the reactions of infancy. The growth of language may begin as a mere manifestation of a need. That is to say, the observer notes the infant acting in a particular way when he is apparently stimulated by some exteroceptive or interoceptive stimulus. This reaction may appear to the observer as a stretching out of the hand or a cry. As it happens, the reaching or crying occurs when the infant is stimulated by objects he cannot reach or otherwise adequately make responses to. The reaching or crying is, then, a gesture or stimulus to the mother or nurse to complete the necessary reaction, and in this manner the reaching or crying of the child becomes a means of finally attaining some result. For a long time the child's activity has no significance to him in the sense that he has no manner of appreciation of its operation. At this stage the action of the child is a bare meaning-act or a means to some other reaction, playing with the ball or drinking the milk.<sup>10</sup> Because of the particular situ-

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<sup>10</sup> We ignore absolutely any standpoint of an observer.

ation this reaction of the child to be sure is pregnant with potentiality to develop into genuine language.

Another stage in the development of language involves the appreciation by the child of the stimulating character of his reaction. This appreciation arises from the observation of the close connection between his own beginning and final reaction, and that of the mother, since the latter's action is a necessary part of the total situation. In many cases this appreciation may merely amount to the fact that the mother has trained the child to substitute a true stimulus word (name) for the original crying or reaching stimulus.<sup>11</sup> Since the development that we are describing consists mainly in the child's learning that his act is a stimulus, the use of a name is not an essential part of the development. Any sort of gesture will do. The entire criterion for the description of the developmental stages in the language reactions lies naturally in the specific ways in which the person is in contact with his surroundings.

Distinctive as a stage in the development of language reactions is the performance of verbal actions as stimuli-determiners of the actions of others. We have already seen that this stimulating act may be merely the expression of a need which is satisfied through the act of another. Later, this stimulus is uttered as a deliberate means of achieving some definite end. Obviously, the best means of accomplishing such purposes is by the use of connected speech-stimuli. "Get-me-this" may be considered as a typical illustration of such a connected speech determiner of action. And it is very important to observe that we have attained here a stage beyond the mere use of name words. In fact we may think of this communicative-stimulus or deliberate, transmissive reaction as a phrasing or speech-expressing reaction which transplants simpler need and want reactions. These connected speech reactions are definitely made to exhibit or express a need, desire, or some other condition of the person, or an implicit response of quite another sort. Such language reactions essentially involve segments of behavior including reaction systems of at least two individuals, either as stimuli or responses.

In this stage of language development words or their equivalents are not mere substitutes for objects or acts but are definite stimuli for one's own reactions or the responses of others. A clear example of communicating with oneself is the use of language in the formulation of plans or the application

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<sup>11</sup> That is, stimulus for the mother.

of pencil marks upon paper during the solution of a mathematical problem.

By far the most elaborate use of communicative language reactions is found in the informational responses which are stimulated to action by another person or his presence. Especially is this the case in informational reactions in which one person apprises another concerning objects or events which have long ceased to be actual factors in the surroundings of one or both persons. By means of communicative language reactions, not only can old events be revived and brought again to the surface, but in general one person may inform another of comparatively new objects or events by way, of course, of stimulating in the other person implicit responses representative of similar conditions.

In general, we might look upon language reactions as instrument reactions; they enable the person to extend his activities and to reach across all sorts of barriers. The use of language reactions puts the human individual upon a definite and characteristic level of action. As instrument reactions, language represents one of the highest developments of the meaning-responses. Not only is language a determiner of overt and implicit reactions of all sorts, but it also serves at the same time as a complex means of developing newer and more capable reactions. We have already suggested that the great efficiency of language acts lies in their capacity to integrate with other reactions and thus form a part of all planning, thinking, and choosing. To a considerable extent language responses constitute the primary factors in the higher types of delayed reactions. We might add in passing that it is the capacity of verbal responses to integrate readily with other reactions, and not any mysterious power, which makes of language such a distinctly human form of behavior.

To obviate any misunderstanding, let us state at once that our remarks concerning language do not apply to printed words or signs, nor perhaps to written words. Since, obviously, printed words or sentences, not being reactions at all, cannot be meaning-responses. They are stimuli pure and simple, although the act of producing written words may be considered to be a meaning-reaction of a highly sublimated sort. In this connection it is well to note that there are distinct levels of action, and that a distinct difference exists between the spontaneous meaning-responses of a personal sort and the deliberative institutional acts constituting the publication of a theory by a scientist or the formulation of a law by a legislature. The latter acts are means to some other acts, of course,

but since they function much less directly they belong to a different level of behavior. Such acts are means to other acts through the instrumentality of institutional or common stimuli, and are not themselves personal stimuli. Written and printed language are common stimuli, and hence are psychological data in no other sense than is any other type of stimulating object. The acts producing these common stimuli are just as indirect means to reaction as are those which result in the construction of a house, or driving an automobile into the path of traffic.

## VII

We conclude, then, that the problem of meanings in psychology involves no other factors than those which are dealt with and described by the ordinary methods of objective science. Not only the simple reactions of the person while in direct contact with objects, but also the most complex thought and memory responses, are definite integrations of responses and stimulating conditions. In other words, the most intricate intentional action may well be considered as an elaborate organization of the person's actions under definite conditions of stimulation. Incidentally, we observe that meaning-responses are not limited to thought processes, but are parts of reaction patterns including all types of reactions. From this list of reactions, habit responses are not exceptions and in fact in every segment of behavior in which there are two or more reaction systems, one serves as a precurrent determining response for others, and therefore answers to our description of a meaning-reaction.

Since precurrent reactions are both overt and implicit, we shall find both types of responses operating as anticipatory determiners of action. For these two types of responses merely represent the different ways in which the person responds to his surrounding stimuli. Our discussion has indicated that the list of precurrent reactions includes not only concepts and images, but also language reactions and other more definitely direct operations upon stimulating objects, namely, those involving the skeletal musculature.



# A QUALITATIVE INVESTIGATION OF THE EFFECT OF MODE OF PRESENTATION UPON THE PROCESS OF LEARNING

By FRANCIS J. O'BRIEN<sup>1</sup>

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## HISTORY

The relation of the mode of presentation of a material to the process of learning it has been approached experimentally from various points of view. Most investigators have sought, with a pedagogical interest, simply to determine the mode of presentation most advantageous in learning; a few have concerned themselves chiefly with the psychological aspects of the problem and undertaken to determine the qualitative changes in the complete mental process that arise when the material to be learned is presented to different senses. Our present problem is psychological in this latter sense. In most studies investigators have been content to deal merely with the quantitative aspects of learning and recall, supplementing such data only occasionally by introspections. They have made the analysis of the learning-process almost entirely with reference to the mode of presentation of the material—whether it is visually or auditorially presented, or reinforced by vocimotor repetition, for example—and little with respect to the actual sensory terms with which the learner represents the material to himself. For this latter information, as well as for a complete understanding of the problem, an introspective study is essential.

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Münsterberg and Bigham<sup>2</sup> published the pioneer investigation in this field in 1894. They presented digits and colors to their observers in visual, in auditory, and in combined visual-auditory fashion, and tested the learning immediately by a method of reconstruction. They concluded that visual memory is superior to auditory and that material presented to both these senses at the same time is more easily reproduced than material presented only to the one or the other.

Cohn<sup>3</sup> took especial account of the motor factor in learning. Using lists of consonants as material, he compared learning with emphasis upon the vocimotor processes with learning with vocimotor processes inhibited. He sought to secure this inhibition by having the *O* press his tongue against the roof of his mouth, count "1, 2, 3, 1, 2, 3," etc., count backwards, or count by 2's or 3's. He concluded that learning was better when there was no attempt to interfere with vocimotor processes. It is possible, however, that his results were due to the distraction of attention by the inhibiting task.

Quantz,<sup>4</sup> in a study of the psychology of reading, performed memory experiments with visual-vocimotor, auditory, and visual-auditory-vocimotor modes of presentation. He concluded that lip movement is a serious hindrance to the speed of reading and that a combination of the visual and auditory modes of presentation has little advantage for recall over either mode of presentation separately.

Lay<sup>5</sup> studied the mode of presentation most effective in learning spelling, and demonstrated that visual presentation is much more effective than auditory and that the introduction of motor imagery is of considerable assistance in the learning processes. Fuchs and Haggenmüller<sup>6</sup> and Itschner<sup>7</sup> repeated Lay's experiments with certain variations and in general substantiated his findings.

Smedley<sup>8</sup> investigated the memory span of children for five different modes of presentation. He found that he obtained the greatest span with the visual-auditory-vocimotor mode, and that his other modes could not be completely realized because his subjects were unable entirely to repress their vocimotor processes.

Finzi<sup>9</sup> worked upon observational noting and retention as conditioned upon presentative modes. He worked without consideration of the imaginal types of his *O*'s, but concluded that vocimotor and manumotor imagery are least efficient for retention. Some of his *O*'s,

<sup>2</sup> Münsterberg, H., and Bigham, J. Memory. *Psychol. Rev.*, 1, 1894, 34-38.

<sup>3</sup> Cohn, J. Experimentelle Untersuchungen über das Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses. *Zsch. f. Psychol.*, 15, 1897, 161-183.

<sup>4</sup> Quantz, J. O. Problems in the psychology of reading. *Psychol. Rev. Mon. Sup.*, 2, No. 1, 1897, 51 pp.

<sup>5</sup> Lay, W. A. *Experimentelle Didaktik*. 3 ed., 1910, xvi+661 pp.

<sup>6</sup> Fuchs, H., and Haggenmüller, A. Studien und Versuche über die Erlernung der Orthographie. *Sammlung von Abhandlungen aus dem Gebiete der pädagogischen Psychologie und Physiologie*, II, 1898, 63 pp.

<sup>7</sup> Itschner, H. Lay's Rechtschreib-Reform. *H. Jahrbuch d. Vereins f. wissenschaft. Päd.*, 32, 1900, 206-234.

<sup>8</sup> Smedley, F. W. *Report of the department of child study and pedagogic investigation*. Chicago, No. 3, 1900-01, 63 pp.

<sup>9</sup> Finzi, J. Zur Untersuchung der Auffassungsfähigkeit und Merkfähigkeit. *Psychol. Arbeit.*, 3, 1900, 289-384.

however, remembered best when motor processes were introduced in addition to the others in the learning.

Kemsies,<sup>10</sup> in a study of school children, concluded that visual presentation was more advantageous than auditory for learning German-Latin vocabularies. Fränkl<sup>11</sup> studied the same problem with more modes of presentation and concluded that Kemsies' conclusion holds for learners of the visual type but that auditory presentation is more efficient for learners of the auditory type. Schuyten,<sup>12</sup> on the basis of a similar experiment, concluded that 'pure auditory' presentation is superior to 'visual plus auditory' presentation. His 'pure auditory' presentation was, however, presumably auditory-vocimotor, and his experiment was made without reference to the imaginal type of the school children.

A more thorough investigation of this problem is Pohlmann's.<sup>13</sup> He worked with six modes of presentation and three intervals of recall. His general conclusions are that auditory presentation is slightly more advantageous than visual for such familiar material as meaningful words, that visual presentation is superior to auditory for nonsense syllables, and that retention is not improved by the addition of the auditory mode or the vocimotor mode of presentation to the visual.

Segal<sup>14</sup> worked with trained O's and with visual and auditory presentation. In both cases he allowed the learner in certain series to use the visual-auditory mode with any imaginal supplementation that he desired, while in other series he required him to avoid vocimotor processes by carrying on simultaneously with the visual or auditory presentation some other irrelevant vocimotor process. He concluded that the same individual may be said to belong to different imaginal types when reproducing different materials, that optimal conditions for reproduction consist of the presentation of the material in the mode that corresponds to the O's type, and that, when presentation is made in a mode different from the O's type, the O either may learn less efficiently using the terms in which presentation is made or may immediately transpose the presented material into imagery corresponding to his type. The necessity for taking strict account of the O's type under the particular experimental conditions and the need for a persistent appeal to introspection become apparent in this study.

Von Sybel<sup>15</sup> reinforced Segal's conclusion by an experiment involving six presentative modes. He noted especially that the imaginal type of the learners had to be considered before the most effective

<sup>10</sup> Kemsies, F. Gedächtnisuntersuchungen an Schulkindern. *Zsch. f. päd. Psychol.*, 2, 1900, 21-30; 3, 1901, 171-183.

<sup>11</sup> Fränkl, E. *Ueber Vorstellungs-Elemente und Aufmerksamkeit. Ein Beitrag zur experimentellen Psychologie.* 1905, 256 pp.

<sup>12</sup> Schuyten, M. C. Experimentelles zum Studium der gebräuchlichsten Methoden im fremdsprachlichen Unterricht. *Exper Päd.*, 1906, 3, 199-211.

<sup>13</sup> Pohlmann, A. *Experimentelle Beiträge zur Lehre vom Gedächtnis.* 1906, 191 pp.

<sup>14</sup> Segal, J. Ueber den Reproduktionstypus und das Reproduzieren von Vorstellungen. *Arch. f. d. ges. Psychol.*, 1908, 12, 124-236.

<sup>15</sup> Sybel, A. von. Ueber das Zusammenwirken verschiedener Sinngebiete bei Gedächtnisleistungen. *Zsch. f. Psychol.*, 53, 1909, 257-360.

mode of learning could be established, and that imaginal type is not fixed, but subject to change under different conditions.

Abbott<sup>16</sup> studied the nature of the mental processes involved in learning to spell unfamiliar English words. Her work was undertaken in order to test the results of Lay, Fuchs and Haggemüller, and Itschner. She found, under her conditions, that the initial recall came typically as visual imagery of the letters of the word no matter what the mode of presentation, that the subject would proceed to pronounce the word only as soon as a clear visual image was obtained, that auditory presentation gave rise, by way of visual imagery, to an even more purely visual learning than did visual presentation, and that the mode of presentation appears, therefore, to determine the imaginal terms of the learning in only a small degree.

Meumann's<sup>17</sup> conclusions support the general trend of the foregoing experiments: it is more advantageous for a learner to use imagery corresponding to his imaginal type than for him to attempt the use of imagery in other modalities. Meumann holds that learning is dependent more upon the formation of strong associations than upon the formation of many associations and that therefore the material which is most readily impressed should be used. Frankfurter and Thiele<sup>18</sup> also came to this same conclusion, although they noted especially that the addition of other modes to the natural mode for the *O* increased efficiency of learning. For them the best results were obtained with visual-auditory-vocimotor learning.

#### MATERIALS, APPARATUS, PROCEDURE AND OBSERVERS

In the first half of the investigation we used significant words; in the second half, nonsense-syllables.

##### *Significant Materials*

This material consisted of four-letter English words,—nouns, pronouns, adjectives, verbs and adverbs,—which were arranged in 36 lists of 20 words each of approximately equal difficulty. The lists were divided into three series, *a*, *b*, and *c*, of 12 lists each.

Twelve modes of presentation were employed and in the order given below. The following symbols are used in this paper in abbreviation of the names of the modes of presentation: V=visual, A=auditory, M=vocimotor, m=manu-motor.

1. *Auditory (A)*. *E* pronounced the words, one every 3 sec., to the beat of a metronome. The learner was asked to inhibit vocimotor imagery during the learning.

<sup>16</sup> Abbott, E. E. On the analysis of the factor of recall in the learning process. *Psychol. Mono.*, 11, 1909, 159-177.

<sup>17</sup> Meumann, E. *Vorlesungen zur Einführung in die experimentelle Pädagogik und ihre psychologischen Grundlagen*, II, 1907, vii+467pp.

<sup>18</sup> Frankfurter, W., and Thiele, R. Ueber den Zusammenhang zwischen Vorstellungstypus und sensorischer Lernweise. *Zsch. f. Psychol.*, 62, 1912, 96-131.

2. *Auditory-vocimotor* (AM). The same procedure as in the purely auditory presentation (A), except that the learner was allowed the use of vocimotor imagery.

3. *Visual* (V). The series as they were presented visually on an exposure apparatus, one word every 3 sec. The learner was asked to inhibit vocimotor imagery.

4. *Visual-vocimotor-auditory* (VAM). The words were presented to the learner as in the purely visual presentation (V), and as soon as he perceived each word he was required to pronounce it aloud, definitely and distinctly.

5. *Auditory-manumotor* (Am). The same procedure as in the purely auditory presentation (A), except that the learner was required in addition to write the words. The learner was blindfolded, so that he could not see the word he had written.

6. *Visual-auditory-manumotor* (VAm). The procedure here is a combination of the purely visual (V) and the auditory-manumotor (Am) modes of presentation, except that the learner was not blindfolded. (See 7, VMm below.)

7. *Visual-vocimotor-manumotor* (VMm). The learner saw the word and was permitted to repeat it in vocimotor imagery but not aloud; and then wrote it. The O's hand was under a screen, so that he could not see the word that he had written.

8. *Visual-auditory* (VA). The learner saw the word, and at the same time heard it pronounced by E. He was required to inhibit vocimotor imagery in the learning.

9. *Visual-vocimotor* (VM). The learner saw the word and was allowed to repeat it in vocimotor imagery but not aloud.

10. *Auditory-vocimotor-manumotor* (AMm). The procedure is the same as in the auditory-manumotor mode of presentation (Am), except that the learner was allowed the use of vocimotor imagery.

11. *Visual-manumotor* (Vm). The procedure is the same as in the visual-vocimotor-manumotor mode of presentation (VMm), except that the learner was asked not to employ vocimotor imagery in the learning.

12. *Visual-auditory-vocimotor-manumotor* (VAMm). The learner saw the word as it appeared in the aperture, pronounced it aloud definitely and distinctly, and then wrote it; he did not perceive visually the word that he had written.

In the visual presentation the list of words was typewritten on paper fastened to a revolving drum which pre-



sented one word every 3 sec. A space indicated the end of the list.

In the auditory presentation a 3-sec. interval was allowed at the end of the list before repetition of the list.

A list was re-presented until the *O* signalled that he had learned it. The *O* would then immediately recall the series to *E*, who would record the recall, the number of presentations required for learning, the time required for recall, and the number and the nature of the errors made.

We sought in this investigation both a *quantitative determination* of reproduction after a few seconds and after 24 hours, and an *introspective account* of the mental procedure in the learning and in both the immediate and delayed recalls. Thus we hoped to obtain a clearer insight into the act of learning itself, and to clear up some of the differences of experimental results that are to be found in the literature.

Seven graduate students in experimental psychology served as *O*'s. They were all highly trained in introspection. Four of them took part in the entire investigation.

#### *Non-significant Materials*

In the second half of the investigation we used 33 lists of nonsense-syllables. Twenty syllables constituted a list. The procedure was identical with the procedure of the first part with two exceptions. In the first place, the visual-auditory-manumotor presentation (VAm) was not employed because the learner's procedure was almost the same as with the visual-auditory-vocimotor-manumotor presentation (VAMm). In the second place, the *O* could see the word as he wrote it. An apparatus was arranged so that the syllable written by the learner was drawn out of sight simultaneously with the coming-in of the new syllable. We employed for this purpose an endless band of paper which passed under an aperture cut in the table and was actuated by electrical contacts controlled by the exposure apparatus, a Marx machine. Thus we prevented continued visual stimulation.

#### *Ideational Equipment of Observers*

*Observer P.* *P* was of mixed type, with preference for vocimotor imagery, but he was able to use visual imagery readily. When vocimotor imagery was prohibited by the instructions, he reported that it was nevertheless present in most of the learning process, although under certain conditions it could be inhibited. His recalls were always in vocimotor imagery.



*Observer C.* *C* was of the mixed type, but dominantly visual. He used vocimotor-auditory imagery, especially when the material was difficult. In anticipating and recalling, he relied mostly on visual imagery, both of the printed word and in his own handwriting. *C* also employed a visual schema of the list of words frequently in learning and always in recall.

*Observer Fe.* *Fe* was of extreme motor type; in the entire investigation he reported only one or two visual images and these were subsequent upon vocimotor imagery. In no instance was he able to inhibit vocimotor imagery. He always recalled the material by means of vocimotor-auditory imagery or by vocimotor innervation. Frequently he employed a kinaesthetic schema of localization.

*Observer T.* *T* was of the mixed type, but preferred vocimotor imagery. He possessed, nevertheless, a good command of visual imagery and recalled many words in visual-verbal terms. When instructed to inhibit vocimotor imagery, he never succeeded; and he always anticipated the material in vocimotor terms. In cases of difficulty of recall, he employed either a visual or a kinaesthetic schema of localization. All the recalls, regardless of the mode of presentation, occurred mostly in vocimotor and vocimotor-auditory terms, accompanied often by visual-verbal or visual-concrete imagery or meaningful associations.

*Observer Fi.* *Fi* was of the balanced type; she habitually employed vocimotor (auditory) imagery alone in learning but she sometimes used visual imagery alone, especially under the *Aufgabe* to inhibit vocimotor processes. In these latter cases, however, the vocimotor processes were present during the first few presentations. She formed many meaningful associations during the learning. The syllables were generally recalled in vocimotor (auditory) imagery; but visual imagery was present in a few of the immediate recalls. She also employed a visual schema of localization. One olfactory image was reported by *Fi*, the only such image reported in the investigation.

*Observer W.* *W* was of the extreme visual type and in learning and recalling he employed a greater amount of visual imagery than of any other kind. He used vocimotor and vocimotor-auditory imagery to no great extent. Even when permitted vocimotor imagery, he employed it only in the first few presentations. He also employed a visual schema of

localization. He found it impossible to inhibit vocimotor imagery in the first few presentations of a new material.

*Observer S.* *S* was of the mixed type with a slight preference for the vocimotor imagery. He was able to use visual imagery in learning, but it appeared only after the vocimotor imagery had been present in the first few presentations. In spite of the use of several contrivances, he found it impossible to inhibit vocimotor imagery under instruction. He employed a visual schema of localization.

## RESULTS

### *Summary of Introspections*<sup>19</sup>

A survey of our introspections brings to light the following points:

I. Vocimotor imagery can not be inhibited, at least not in the first part of the learning.

*P, VA.* "During the presentation I focused attention on your voice, adjusting my right ear toward you and holding the auditory stimulus as long as possible; I attempted to anticipate, but when I succeeded the words always appeared in vocimotor imagery."

*T, V.* "In the fore-period there were sensations of pressing my lips together, pressing the tongue against the teeth, kinaesthesia of strains in the muscles of the stomach, and general muscular inhibitions throughout the whole body. This tense state seemed to be an attempt to inhibit vocimotor imagery; but in spite of it all the first presentation of each syllable was followed by a vocimotor-auditory repetition of it."

*C, A.* "In the first part of the learning I found that if I attended focally to the perception of each word and then to auditory imagery of it afterwards, I could not help having vocimotor imagery of it; so in the first 5 presentations at least I had rapid vocimotor imagery of each syllable after it was presented."

*W, VA.* "As soon as the first word was presented I found myself repeating it in vocimotor imagery, then awareness of the *Aufgabe*, present in visual imagery of you. . . . When the next word was presented I found myself repeating it again in vocimotor imagery."

*Fi, A.* "In general I would repeat the syllable and the associated word in vocimotor-auditory imagery, and at the same time I was keenly aware of tensions about my tongue and throat, tensions which would increase at that moment. In some cases the vocimotor image was very slight."

*S, VA.* "As soon as you said, 'Inhibit vocimotor imagery,' I was conscious of a numb feeling in the vocal apparatus. In the first presentation I was aware of a vocimotor image of the word as soon as I received the visual percept. . . . This vocimotor image after

<sup>19</sup> In the following excerpts from the protocols, the letter at the beginning of the paragraph indicates the *O*, and the symbol following the mode of presentation: see p. 252.

the presentation of each word was present, I am certain, during the entire first presentation and occurred very many times during the second presentation."

II. Observers of motor type never wholly succeed in eliminating vocimotor imagery in the learning, even when instructed to do so.

*T, Am.* "After the presentation of each syllable I had vocimotor and auditory repetition of it throughout the whole learning; I prevented movement of lips and actual throat movement, but I could not repress a sensation of movement in the tongue, and auditory imagery often accompanied this vocimotor imagery."

*Fe* was never able to inhibit vocimotor imagery; it was present in all the learning with all materials.

III. With *O*'s of visual type, the words are anticipated in vocimotor imagery; gradually the words appear to consciousness more and more in visual and less and less in vocimotor imagery; finally the words are anticipated in visual imagery only and no vocimotor imagery of the words is present.

*C, V.* "In the first part of the learning I was unable to rule out vocimotor-auditory imagery; it immediately followed the visual perception of the word. But as the learning went on the amount of vocimotor imagery became less and less. In the last 6 or 7 presentations there was almost no vocimotor-auditory imagery; the words were anticipated almost wholly by means of visual imagery."

*W, VAm.* "Vocimotor imagery appeared very seldom after the first 3 or 4 presentations, the anticipation being done mostly in visual imagery."

IV. If *O*'s of visual type attempt to inhibit vocimotor imagery, visual imagery in the learning sometimes becomes less clear; in the recalls the visual image develops more slowly than it does when vocimotor imagery is not interfered with.

*C, A.* Delayed Recall. "All the syllables came in visual imagery but very slowly and in no definite order. . . . Only 4 or 5 of the syllables were definitely localized. Strains in my eyes and consciousness that the words were coming very slowly."

*W, AMm.* "None of the visual imagery was very clear. Accompanying the learning were great strains and unpleasantness; I could not give the words fast because they came in so slowly."

*W, Am.* *W* gave up his attempt to learn the series after it was presented to him 18 times; he said, "I could not learn this series because I was unable to visualize the words as this is the medium I use in learning."

V. In addition to the help obtained from meaningful associations, there is another aid which comes from the vocalization of the words and which we may call a 'motor or kinaesthetic help': on pronouncing the first word the *O* may find his vocal apparatus set to pronounce the following word.

T, VM. Delayed Recall. "The syllables before SEC came in very quickly, one after the other, in vocimotor imagery; that is, after pronouncing one word I would find my vocal apparatus set to pronounce the next, and this word was then immediately vocalized. As soon as I vocalized VIT I found my vocal apparatus about to pronounce CAY."

C, V. "As soon as I recalled POM I was conscious of eye-movement downward, and vocimotor and auditory imagery of POM; before I finished vocalizing POM the vocal apparatus was adjusted for the pronunciation of TER, and POM-TER were pronounced very quickly, one after the other."

Fe, A. Delayed Recall. "The vocal apparatus was set for the pronunciation of a hard *c*-sound. Then I found myself pronouncing COCO."

Fi, VAM. Delayed Recall. "WAB flashed in mechanically in vocimotor innervation; after the vocalization of VOQ (which immediately preceded WAB) my vocal apparatus formed for the vocalization of WAB."

S, Am. Delayed Recall. "After vocalizing TALL I was conscious of the adjustment of the vocal organs for the *ive*-sound; this was followed by a visual image of FIVE in my own hand-writing."

VI. If given the *Aufgabe* to inhibit vocimotor imagery, the O sometimes sets up for himself a new *Aufgabe* to use imagery of another modality.

P, VA. "I tried to anticipate, but when I did I used vocimotor imagery, so I stopped anticipating; then I set up the vocimotor-auditory *Aufgabe* to retain the words in visual concrete images."

T, VA. "I tried to anticipate, but I found myself using vocimotor imagery. . . . I set up the vocimotor and auditory *Aufgabe* to anticipate in visual imagery but I did not succeed; vocimotor imagery always coming in."

C, VA. "There was little tendency to use vocimotor imagery after the presentation of each word; I set up the vocimotor-auditory *Aufgabe* to anticipate in visual imagery; I just sat and looked as the words were being presented, attending very little to the auditory presentation."

W, VA. "I then found myself seeking for a way to fulfil the *Aufgabe* [to inhibit vocimotor imagery]; I had a visual image of a sheet of paper and myself; then a vocimotor and auditory, 'Attend to the visual image of the words; never mind the vocimotor image.'"

S, A. "I was using vocimotor imagery in learning these words; I then set up the vocimotor-auditory *Aufgabe* to substitute the exhaled breath for the vocalization of the word; the vocimotor imagery became less, and I had a second *Aufgabe* to use visual imagery after you spoke the word."

S, Am. "I set up the vocimotor-auditory *Aufgabe* to visualize the words in my own hand-writing; this I did, but I was not able to anticipate in this visual imagery."

VII. Any attempt to inhibit vocimotor imagery makes auditory imagery of the word more clear and intensive.



*T, Vm.* "Following the visual percept of the word there was a vocimotor-auditory image of it. The vocimotor image was much less intensive than the auditory. The auditory image of the word became much more intensive as I was able to suppress the vocimotor."

*C, Vm.* "Later on in the learning the vocimotor seemed to drop out and the auditory image seemed much louder; the vocimotor image almost completely dropped out in the last half of the learning."

*Fi, VA.* "I am quite sure that I inhibited vocimotor innervation of each word, but there was slight vocimotor imagery and very intensive auditory imagery."

VIII. In a visual-auditory presentation an *O* seldom attends to both the visual and the auditory stimuli; he attends either to the visual alone or to the auditory alone.

*P, VAm.* "During the presentation of the words I paid the least possible attention to your pronouncing . . . concentrating attention almost wholly upon the visual perception."

*C, VA.* "During the middle of the series I was not attending to your voice at all but to my own anticipatory processes and to my retaining, in visual imagery, of the last two words which I had just perceived visually."

*Fe, VA.* "My attention was directed much more to the auditory perception than to the visual perception of the word; in fact, once or twice I found that my eyes were actually closed, while I was perceiving the words in auditory fashion only."

*W, VAm.* "For 4 or 5 presentations I paid little attention to your voice; it was by no means in focal perception for at least 4 or 5 presentations; I attended mostly to the visual perception."

IX. In an auditory presentation all *O*'s had great difficulty in distinguishing syllables containing such letters as *d, t, q, c, k, etc.*

*T, Am.* "After you pronounced QAZ I wrote it KAZ; the *k* held my attention. Vocimotor-auditory imagery, 'That must be a *q* because there is a *k* later.'" *T* had to reason, therefore, as to the correct spelling of a syllable; the auditory perception made possible at least two spellings.

*C, AMm.* "During the first few presentations the auditory perception was not instantaneous; not definite or clear; that is, not so much the clearness of the sound but rather a lack of definiteness of the letters pronounced. For example, for one syllable I write it at least two ways: BER and BUR."

*Fe, AMm.* "In the first presentation several words presented were capable of two interpretations; *e.g.*, BARN-BARM. This caused nervousness."

*W, A.* "I had great difficulty with the syllables containing either the letters *c* and *q, d* and *b*; after perceiving a syllable containing such a letter there was confusion as to what the exact spelling was; this confusion consisted mostly in a fluctuation of the two possibilities in visual imagery."

*Fi, AMm.* "Sometimes I had difficulty in perceiving the word definitely; I would find my vocal apparatus set to pronounce but for no



definite syllable; this setting of the vocal apparatus only appeared when I was not able definitely to perceive a syllable.

S, AM. "In 3 cases it was very difficult for me to get a distinct perception of the words, and I was not able to vocalize them easily; there were elements in my auditory perception of your voice in pronouncing the words which I was unable to reproduce."

X. If, in recalling a series which had been presented to the O in auditory fashion, auditory imagery is present, it is seldom of E's but usually of O's voice.

C, AMm. "The words were recalled in vocimotor-auditory imagery of my own voice; then a visual image of each word in my own handwriting."

Fe, A. "In the immediate recall I made use, as far as I am aware, only of vocimotor-auditory imagery; the auditory imagery was of my own voice."

W, AMm. Delayed Recall. (What was the nature of your auditory imagery? Was it of your voice or of my voice, or of any one else's voice?) "The auditory imagery when it appeared was always of my voice."

Fi, A. "I anticipated every syllable in auditory imagery of my own voice, perhaps a little vocimotor, sometimes 2 or 3 ahead of the actual presentation."

S, VA. Delayed Recall. "I gave the first 3 words from visual imagery, but the word SOIL (the second word of the three) was followed by auditory imagery of my own voice."

XI. Manumotor imagery does not help either in learning or in recalling a list of words or nonsense-syllables.

P, VAm. (Did any manumotor imagery appear in the learning or the recalling of this series?) "I had absolutely no manumotor imagery at all."

T, Am. "The immediate recall of this series was almost wholly in vocimotor-auditory imagery, the auditory being of my own voice; there were no visual and no manumotor imagery present."

C, Am. "I anticipated far ahead of the drum, not attending to your pronunciation until I failed to anticipate; there was no consciousness of any manumotor imagery whatever."

Fe, Vm. (Did any manumotor imagery enter into the learning or the recall of this series.) "No, none at all."

W, VAm. "In anticipating I always set up the *Aufgabe* to call up the words visually; if a visual image of the word failed to appear I attended to my arm; but no manumotor image of a syllable ever appeared."

Fi, Vm. Delayed Recall. "There was no consciousness of any manumotor imagery; in fact, there was no consciousness that I ever wrote the material."

S, Am. "Before I wrote a word I was never conscious of either a visual or a manumotor image of it."

XII. In recalling words between which associations have been established in the learning, the imagery of these words

may appear very quickly, and the association is present only inasmuch as the words appear more quickly.

*P, AMm.* "The first word of an associated group would appear and the remaining words would come more readily with very little attention, one after the other."

*T, AM.* "All the syllables came very quickly, one after the other, but there was no consciousness of their meaningful connection; there seemed to be no meaning attached to the words excepting the fact that they came very quickly."

*C, VA.* Delayed Recall. "There was no visual concrete imagery with the word NUNS; i.e., the visual concrete imagery that appeared to consciousness during the learning process; but as a result of the association made with NUNS and CAPE, the word CAPE came more quickly in auditory imagery after the word NUNS, than was the case where two succeeding words in the series were not associated."

*Fe, Vm.* Delayed Recall. "The meanings were present in the words with which associations had been formed in the learning, as far as I can see, only inasmuch as these words came together."

*W, VMm.* Delayed Recall. "The meaning which was present in the learning of the groups was not clearly present, the effect being that these words appeared very quickly, one after the other."

*Fi, VA.* Delayed Recall. "After a few presentations the associations did not come in with the syllables; the syllables themselves seemed to function just as the meaningful words would have done in carrying meaning."

XIII. When associations have been formed with the words of the series the following phenomenon often appears in the recall: The meaningful association comes first; the actual words of the series come later.

*P, Vm.* (Did any associations appear in the immediate recall?) "Yes. With the words DOTH-KLINK-GLOW the meaningful content came first and the words themselves later."

*T, AM.* Delayed Recall. "The feeling of familiarity was present with most syllables and the meaningful association came in before the nonsense-syllables."

*C, VM.* Delayed Recall. "I set up the vocimotor-auditory *Aufgabe*, 'Try to call up an association and see if the words will come;' immediately 'wigwam' flashed in in visual imagery, but the 'wig' faded out of consciousness very quickly and WAM was clear and distinct."

*W, Vm.* Delayed Recall. "The associations which I had in the learning came in before the words and were followed in each case by visual imagery of the word; the associations always came in in visual imagery."

*Fi, VAM.* Delayed Recall. "I had a vague visual image of some books; this meant for me language-books. Then came the vocimotor-auditory image 'Latin;' immediately QAH appeared in visual imagery."

*S, VA.* Delayed Recall. "In anticipating the series the concrete imagery came in first; e.g., I had visual imagery of some soil and a consciousness, which I can not describe, that the next word was related to soil; following this I had a visual image of a tub of clams; imme-

S, A. Delayed Recall. "The first word came on the top of a visualized sheet of paper, the visual image being of the mimetic sort (f) and accompanied by vocimotor and auditory imagery of 'ent.' Immediately DENT was vocalized."

XIX. During the act of anticipating, and later in recalling a series, a kinaesthetic schema of the series of words or syllables is sometimes present, especially with O's of the motor type.

P, AM. "Sometimes I stopped anticipating between the pairs and attempted to get a clear percept of each word, localizing them on my fingers; sometimes I did this for two successive presentations."

T, AM. "The immediate recall was as usual in vocimotor imagery with kinaesthetic imagery of tapping in the tempo with which each syllable was presented in the learning."

Fe, VAMm. "There was also present my localizing kinaesthetic schema, present in kinaesthetic imagery of pointing from left to right with my right hand, with eye-fixation at the place where I was pointing. In this process the eye-fixation was much more focal and intensive than the kinaesthetic imagery of pointing."

W, VAM. "Eye-kinaesthesia. I had a visual image of a sheet of paper; then unpleasantness and a sudden shift of visual attention to the bottom of the paper, which meant to me that the words which I had just now recalled did not belong at the top of the list."

Fi, AM. Delayed Recall. "For the first time in the recall I was aware of a vague visual schema which was different from the one I generally use; that is, I was aware of certain regions in the air in front of me, more of a spatial reference to something that was not filled in. This was followed by a kinaesthesia in the neck and eyes of turning slightly to one side in order to fixate this region; perhaps there was actual eye-movement. . . . Then DOY came in in visual imagery."

S, A. "I was conscious of eye-movement up and down this visual schema; the lower part of the schema was focal and the first 3 words were anticipated in visual imagery."

XX. Words which are not recalled promptly are generally preceded, when they are recalled, by a schema which mediates their advent into consciousness.

P, VMm. "When a word did not appear I would focus attention on the finger with which that word was associated; and the word when it appeared would come slowly in vocimotor and auditory imagery."

T, Am. Delayed Recall. "Attention as focused on the bottom of a visual image of a vertical list. Eye-movement up several times, which meant to me 'Start at the bottom and go up.' While fixating this point, QIW finally came in in vocimotor imagery, but there was no visual imagery."

C, A. Delayed Recall. "Before I recalled KITE and SOFA, I had a visual blank in which there was room for 2 words; then vocimotor-auditory imagery, 'Two words associated together;' then both words appeared in visual imagery, localized on this visual schema, one over the other."

*Fi, A.* "There was a hesitation in which no words came; great tensions in the body; there had been gradually developing a visual schema in which I saw the series in a grayish outline extending toward the south-west from the floor."

XXI. If the word is recalled in imagery of the modality corresponding to *O*'s type and is not clear, it is often clarified by a coming in of imagery of a modality corresponding to the mode of presentation.

*T, VA.* Delayed Recall. "DEJ appeared in vocimotor-auditory imagery, followed by a visual image, localized to the left of the aperture; the *d* was especially clear."

*C, A.* Delayed Recall. "In recalling DEAD, DEA appeared first in visual imagery, and this was completed by an auditory image forming the complete word DEAD."

*W, Am.* Delayed Recall. "CARD came in in visual imagery, localized at the top of the visual schema; not more than the *AR* and a very faint *d* were present. Immediately I had auditory imagery of the whole word, with emphasis upon the *c*; I repeated the whole word to myself in vocimotor-auditory terms several times."

*S, VAMm.* Delayed Recall. "I had a faint vocimotor-auditory image of TOLA. This was followed by a very clear visual image of it in print."

XXII. The recalled word may appear first in the imagery corresponding to the mode of presentation and may be completed or clarified by the coming in of imagery corresponding to *O*'s imaginal type.

*T, AM.* "After a long pause I had a clear visual image of the letter *a*; then a vocimotor image of FAZ; then I vocalized it."

*C, AM.* Delayed Recall. "Very often I had a visual image of the first part of the word which would be completed by a very clear distinct visual image of the whole word."

*W, A.* "XUT appeared first in indistinct auditory imagery, I think of your voice; this was completed by a visual image of the word on a sheet of paper in front of me; the word appeared in type."

*Fi, Vm.* Delayed Recall. "All the words came in. There was perhaps a very vague visual image first, in printed type, accompanied by a distinct vocimotor-auditory image."

*S, VA.* "Most of the syllables were usually completed or filled out by a vocimotor image, although the syllable first appeared in a visual image."

XXIII. Visual imagery is not suited for a rapid anticipation or recall. If the *O* recalls very rapidly, it is found that the visual imagery does not develop quickly enough, and that for this reason the *O* often has recourse to vocimotor or auditory imagery or to both.

*C, VAM.* "I began anticipating the second presentation in auditory imagery, then in visual imagery. About the middle of the presentation I started rapid anticipation and this was done in vocimotor-auditory imagery, the visual imagery not coming in at all; I anticipated so rapidly that it seemed there was not time for it to develop."



*W*, VMm. "In the immediate recall I was surprised to find myself repeating the first 5 words in vocimotor imagery with no visual; I gave them very rapidly from the vocimotor image and only one vague visual image (of the word WAGE) came in."

*Fi*, V. "I then attempted to visualize the words, but the attempt always failed me and I abandoned it. The auditory-vocimotor anticipation was much more rapid than the visual."

XXIV. The mode of presentation does not determine the modality of the imagery which the learner will employ in learning a given material.

*P*, A. Delayed Recall. "The first pair of words that appeared was ERGO-VAMP; they appeared in vocimotor imagery while pressing the fourth finger."

*T*, V. "The immediate recall of this series was mostly in actual speech, many of the words being preceded by vocimotor-auditory imagery."

*C*, AM. "Most all the words appeared in visual imagery, followed very often by vocimotor-auditory imagery; the words were localized on my visual schema."

*Fe*, V. "CALF, SOFT, HUMP, URNS and BARK all came, one at a time, in vocimotor-auditory imagery, most of the auditory being of my own voice, but the vocimotor image seemed to be more intensive and clear."

*W*, AMm. Delayed Recall. "The first 7 words came in visual imagery; no vocimotor or auditory imagery was present."

*Fi*, V. "The immediate recall came in vocimotor imagery; no visual imagery."

*S*, A. Delayed Recall. "This recall was made from a visual image of the list about the size used in the presentation; I started at the bottom of the list and went up. HASH came in vocimotor imagery; then in visual imagery. MILT-CUBE came in visual and vocimotor imagery. Which came first I can not say."

XXV. If the *O* fails in his attempt to anticipate a forthcoming word, he sometimes attends to the preceding words which he had just anticipated, and then he waits for the desired word to come into consciousness.

*P*, VM. "After a few presentations I was able to anticipate the first pair and also the last 5 pairs. After this I attended wholly to the words which I was not able to anticipate. As soon as I perceived them I repeated them over and over until the next word was presented. . . . During this time I was neglecting the last 5 pairs which I had anticipated early in the learning. Later on, when I perceived these words focally as they were being presented, they appeared as new words."

*T*, VAM. "After I was able to anticipate the syllables I would hardly look at them . . . as if I were anxious to get to the place where I could not anticipate."

*C*, VMm. "In the third stage of the learning I attended to the drum only when I was not able to anticipate a syllable or when I was not sure when the word which I had anticipated was correct."



*Fe, V.* "Toward the end of the learning I had visual perception only of those words which I was unable to anticipate."

*W, A.* "Toward the end of the learning process I did not attend to your presentation of the words until I was able to anticipate. In the last presentation I anticipated all the syllables."

*Fi, VAM.* "The syllables were anticipated mostly in vocimotor-auditory imagery; next I would look at the drum and, if I had anticipated correctly, there would be a pronunciation of the syllable and then a turning away of my visual line of regard from the drum; then I would anticipate the next word."

*S, AM.* "In the third presentation I actually anticipated every syllable in the series; this anticipation was made with but little attention to you; I attended only to my own anticipatory processes."

XXVI. If an *O* is unable to anticipate a word he often repeats the preceding words until the desired word appears.

*P, VA.* "All the words came easily up to the seventh pair; then a blank; that is, no imagery came; then I had imagery of the next pair, so I gave it; I then went back to the sixth pair and repeated it in vocimotor-auditory terms; immediately the second pair followed right on."

*T, VAM.* Delayed Recall. "PIW was repeated several times, mostly in auditory imagery. Immediately CUG-QAR came in vocimotor imagery, no schema being present."

*C, VAM.* Delayed Recall. "The first 8 words appeared first as very definite and intensive auditory imagery, followed by vocimotor imagery; then I had a visual image of a blank, to which I attended. Finally NEXT appeared in vocimotor imagery."

*Fe, AMm.* Delayed Recall. "'Crop' came very easily in vocimotor-auditory imagery; auditory imagery of my own voice; then no imagery came but there was great unpleasantness and strains. 'CROP, —un, un' was repeated several times in vocimotor imagery; finally a vocimotor, 'Gee, don't know these.'"

*W, VM.* "When the words did not appear promptly in the immediate recall there was a period during which there was no imaginal content in consciousness; then my eye moved up and down that part of the visual schema to which the desired word belonged; usually visual imagery of the words preceding and following the desired words would come in; then suddenly these words would appear in visual imagery in their correct place."

*Fi, A.* Delayed Recall. "I had a visual schema in which was a blank space for about 2 words; I attended to the schema for awhile, conscious of strains in the whole body; then the syllable NAF came in vocimotor-auditory imagery and the remaining words of the series followed."

*S, VAM.* "I had a visual image of my schema with a visual image of IDOL at the top of the list; then blank spaces for about 3 words below which meant to me that there were words left out; then vocimotor-auditory *Aufgabe*, 'Try your vocimotor; you can get it that way;' then I had vocimotor-auditory imagery of IDOL several times and the remaining words came in finally."

*The Process of Learning*

The process of learning was found to consist of three distinct stages; and these three stages recurred throughout the experiments, whatever the mode of presentation, or the nature of the materials to be learned, or the ideational type (or learning-type) of the learner.<sup>20</sup>

I. *The Orienting Stage.* This initial stage of the process of learning extends over only the first few presentations of any given series to be learned. Whether it is confined to the first presentation alone, or whether it extends over several presentations, depends less upon the ideational type of the individual than upon the nature of the material itself (i.e., whether meaningful words or nonsense-syllables) and the nature of the series (i.e., whether difficult or easy for the particular individual).

During the presentation of the material in this orienting stage, the attitude of the learner is one of passive receptivity. He makes no attempt either to group or to anticipate the words which are being presented; he merely perceives them. There are rare exceptions. Occasionally an *O* manifested an incipient tendency during this initial stage to group the materials into larger units, a tendency apparently consequent upon his having set up for himself during the foreperiod an *Aufgabe* to divide the list of material into groups of definite size. *W* sought groups of five and *Fe* groups of four.

If the mode of presentation is a combined visual-auditory one, the learner in most cases attends focally to either the auditory or the visual stimulus alone, and very seldom to both equally. A feeling of familiarity for the series tends to develop during this initial stage, and marks the progress of the learner's orientation in his task.

It appears that the perception of the words, no matter how they may be presented, is invariably followed, at least in the first few presentations, by a reflex reproduction of them by the learner. This reproduction occurs in vocimotor or in vocimotor-auditory terms. Even the instruction to inhibit vocimotor imagery fails to prevent the vocimotor reproductions, at least during the first few presentations of the list.<sup>21</sup>

<sup>20</sup> Meumann, *Vorlesungen*, II, divides the learning process into four stages: (1) the orientation or adaptation stage; (2) the stage of passive receptivity; (3) the stage in which the material is tentatively recited or checked by the learner; (4) the stage in which the learner finally fixes the uncertain parts of the series. The first and second stages of learning as given by Meumann differ but little, if at all, and therefore can easily be combined into one stage. If this is done our classification agrees exactly with that of Meumann.

<sup>21</sup> Secor, W. B. (Visual Reading, A study in mental imagery, *Amer. J. Psychol.*, 11, 1900, 225-236), in his experiments on reading, reported that vocimotor movements could be inhibited. Curtis, H. S. (Automatic movements of the larynx, *ibid.*, 11, 1900, 237-239), using a different type of laryngograph, obtained graphic records of vocimotor movements when his *O*'s were mentally reciting a poem or a selection of prose. If Curtis' results are to be taken to mean that vocimotor movement was present in all cases, it is probable that Secor's learners employed vocimotor imagery, if not the actual vocimotor innervation. Abbott (On the analysis of memory consciousness in orthography,

When a learner of the visual type is fairly successful in inhibiting vocimotor processes, his visual imagery is usually less clear and definite than when the vocimotor processes are allowed to function in natural fashion. When the learner of the motor type attempts to inhibit the vocimotor processes, auditory imagery frequently makes its appearance and plays a more important rôle than when the vocimotor processes are not interfered with. Auditory imagery may thus be enhanced even when the learner does not succeed in completely inhibiting his vocimotor processes.<sup>22</sup>

In rare instances learners succeeded in almost wholly eliminating the vocimotor processes from the very start of the series; but in every such case they found that it was impossible completely to memorize the materials presented.<sup>23</sup> Even though the list were presented a great many times (in one instance 39 times), the process of memorizing still remained far from complete, and the learner eventually always gave up his attempt to memorize the material. A subsequent attempt to recall the list would show that not more than 5 or 6 words out of a list of 20 had been memorized, and that these words the *O* recalled with but a slight degree of subjective assurance and with no definite consciousness as to their exact position in the list. Every attempt to complete the act of memorizing without the participation of the vocimotor processes thus ended in failure and the abandonment of the effort to learn on account of fatigue or lack of time. Learners of the motor type were especially unsuccessful.

With the *O* of visual type, the vocimotor image, though essential at first, tends to disappear very early in the learning process. It is seldom present after the fifth or sixth presentation of the series. This rule holds even when the learner is not instructed to inhibit vocimotor imagery. We hold, consequently, that, whatever the imaginal type of the learner may be, vocimotor imagery or vocimotor innervation is absolutely necessary for an individual to begin to learn a series

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*Psychol. Rev. Mon. Sup.*, 11, 1909, 127-158) found that vocimotor processes were always an aid, and Smedley (*op. cit.*), concluded that it was impossible to test a single sense-modality because vocimotor imagery could not be repressed. The results obtained by Mould, Treadwell and Washburn (The influence of suppressing articulation on the favorable effects of distributing repetitions, *Amer. J. Psychol.*, 26, 1915, 286-288) show that the recall is twice as efficient when the *O* is allowed to use vocimotor imagery in the learning as when he is told not to use it, and accordingly attempts to inhibit it. The statistical data of these investigations indicate that vocimotor imagery was never wholly eliminated, and that the repression of vocimotor imagery was always a great distraction to the learners.

<sup>22</sup> This phenomenon has also been reported by Abbott and Secor, *op. cit.*

<sup>23</sup> Kline, L. W. (A study in the psychology of spelling, *J. Educ. Psychol.*, 3, 1912, 381-400) found that any interference with the dominant receptor mechanism results in a greater impairment of the learning process than does an interference with the preferred form of expression. Müller and Schumann (Experimentelle Beiträge zur Untersuchung des Gedächtnisses, *Zsch. f. Psychol.*, 6, 1893, 81-190, 175-339) found that the repression of rhythmic vocalization renders learning almost impossible for some *O*'s. Cohn (*op. cit.*) also found that vocimotor processes were important to the learning and that learning was less efficient when the learner attempted to inhibit vocimotor imagery.



of words or nonsense-syllables. In these early stages it can rarely be eliminated by instruction, and its elimination, when it does occur, prevents learning.

When the *O* is given the *Aufgabe* to inhibit vocimotor imagery, he reacts in either of two ways. (1) He may accept the *Aufgabe* and actively attempt to inhibit it; if he is successful to a great degree, very little learning, if any, takes place. (2) He may comply with the instructions by setting up for himself a new *Aufgabe*, usually in vocimotor terms, to use imagery of another modality; e.g., 'use visual imagery.' This latter procedure is more efficient than the first, but is effective only for *O*'s of the visual type and after the first few presentations.

When the *Aufgabe* required that the *O* pronounce the material aloud, vocalization often proved a hindrance to the learning. The chief fault of vocalization lies in the fact that it fills up so much of the time-interval (3 secs.) between the presentation of successive syllables that little time is allowed for the learner to anticipate the next syllable, and such anticipation is the learner's method of testing his knowledge of the series. As a matter of fact vocalization of the material, when required, becomes less intensive in the later stages of the learning.

II. *The Stage of Attempted Anticipation.* In the second stage of the learning process the *O* is very active in his attempts to anticipate the forthcoming members of the series. The power to anticipate the series becomes in every case the *O*'s criterion that he has learned the material.

The anticipation takes place either in visual or vocimotor (auditory) terms, depending upon the learner's type. In this stage the learner actually succeeds, however, in anticipating only a very few words, for he is concerned chiefly in obtaining a clear perception of each word during presentation and in attempting to combine the word at hand with the preceding and subsequent words.

In rapid anticipation the learner of visual type often makes use of vocimotor-auditory imagery, reporting that visual imagery does not develop quickly enough and that he therefore finds himself using vocimotor imagery.<sup>24</sup>

The first two or three and the last two or three words of a series are usually the first to be anticipated. There is no definite order in which the remaining words begin to be anticipated. Only one of our 7 *O*'s (*S*) did not invariably resort to grouping of the words, and even he occasionally used this expedient, although by no means so frequently as the other *O*'s who always grouped the words.

The number of words or syllables which constitute a group is determined in one of five ways. (1) *The time intervening between the presentation of two successive words.* As soon as a word was presented, the learner would repeat it, and then repeat as many of the preceding words as possible before the following word was presented. The maximum number of words that he was able to repeat

<sup>24</sup> Binet and Henri (La mémoire des mots, *Année psychol.*, 1, 1894, 1-23) report material recalled in vocimotor-auditory terms, because recall was so rapid that a visual image did not have time to develop. Von Sybel (*op. cit.*) found that when an *O* of visual type became fatigued he had recourse to vocimotor imagery. Von Sybel also found that the visual learner again employed vocimotor imagery when the presentation of the material was very rapid. Pohlmann (*op. cit.*) observed that an *O* of visual type is hampered in his learning by a too rapid auditory presentation.

in this time would constitute a group. (2) *The immediate memory-span of the O*. As many words as an *O* was able to repeat to himself from immediate memory without confusing their order or forgetting a word would constitute a group. If he attempted to add another word to such a group, he would be unable to recall one or more of the words, and this failure would mean to him that he had exceeded his immediate memory-span. (3) *A meaningful association*. If a certain number of words form a meaningful association, then this number of words may form a group. (4) *The Aufgabe*. In the fore-period, or during the learning, the *O* may set up an *Aufgabe* to group in fours or fives, thus forming a purely arbitrary group. The size of the groups chosen is frequently determined by the *O*'s experience, either during the experiment or in previous experiments, by which he knows what size of group is the most efficient for him to work with in learning a series of words. (5) *Difficult words*. A difficult word often marked the beginning of a group; if a learner had especial difficulty either in perceiving a word or in anticipating it, this difficult word would become the initial member of a group. The next difficult word would be the first word of the next group; thus the number of words which would constitute such a group varied.

A group formed through the medium of a meaningful association is least apt to be forgotten in the recall. An entire group thus formed may fail to appear to consciousness in the recall; but, if the first word of the group can be recalled by the *O*, the others follow very quickly.

Meaningful associations are present in the first few presentations only and then drop out, seldom to reappear in the learning. This rule holds no matter in what modality of imagery the association appears. The effect of such association is manifest in anticipation and recall, for the meaningfully associated words come into consciousness more quickly, one after the other, than the other words.

The temporal relations between the appearance of the words of the series and their meaningful associations are as follows. (1) The words to be recalled come into consciousness quickly and clearly in the imagery of the *O*'s type; the meaning is present in the words themselves and in the fact that they come together. The *O* was unable to find any other introspective evidence to explain the meaning. (2) The meaningful content comes into consciousness in imagery of any modality; the words of the series come later and very quickly, one after the other, in the imagery of the *O*'s type. If the association is present in a visual-concrete or an auditory-concrete image, it is followed by verbal imagery of the desired word in the imagery of the *O*'s type. (3) The words themselves come first in the imagery of the *O*'s type, and the meaningful content comes later, either in verbal or concrete imagery of any modality. (4) If a meaning was attached to a syllable by adding one or two syllables to the presented syllable, thereby making it a meaningful word, the *O* 'knows' what part of the made-word is the syllable desired by the fact that the desired syllable is more clear and definite, if a visual image, and more intensive and distinct, if the image is motor or auditory or auditory-motor, than the added or associated part.

III. *Anticipatory Stage*. In this final stage of the process of learning, the *O* is concerned chiefly in anticipating the syllables. He may anticipate as rapidly as he is able with no reference to the words which are being presented, or he may anticipate one word at a time just before it is presented to him. As soon as the word is presented he



perceives it focally, and again anticipates the next word just before it is presented to him. Vocimotor imagery may or may not be present if the *O* is of visual type; if he is of motor type he repeats this stimulus-word several times and then links it with the preceding or subsequent words, or with both.

In the case of an *O* whose anticipation of words or syllables is far in advance of their actual presentation, an interesting phenomenon is seen when the *O* comes to a point in the series where anticipation is impossible. Then the process of anticipating ceases until, in the actual presentation, that word is reached which he failed to anticipate.

This procedure of anticipating and linking up the words which he can not anticipate, a learner continues until all the words have been anticipated at least once. In many instances, after correctly anticipating the series once, he will set up the *Aufgabe*, 'Go through and anticipate the series once more, to make certain;' and then, having again anticipated successfully, he will signify that he has learned the series.

When the material is presented to the learner in a purely auditory fashion, he very often finds it difficult to obtain a definite percept of words or syllables containing such letters as *c*, *k*, *d*, *q*, and *t* occurring either as initial or final letters, and *w* and *h* occurring as final letters in a syllable.<sup>25</sup>

Some *O*'s, as soon as they perceive the auditory stimulus, decide upon a definite spelling of the word, especially in the latter part of the investigation. In this way the uncertainty as to the correct spelling of the word, if it enters at all, is very slight. In most instances such words are not spelled by the *O*'s in the form in which *E* had them in his lists.

Greater attention, it appears, may be secured by material presented in auditory fashion than by material presented visually. This difference is due to the nature of the auditory presentation itself. Individuals find it more difficult to obtain a definite auditory perception

<sup>25</sup> Henmon, V. A. C. (The relation between mode of presentation and retention, *Psychol. Rev.*, 19, 1912, 79-96) found that material was learned more efficiently when the words were presented in purely auditory fashion, but he excluded all syllables ending in *c*, *q* and *h*, and all syllables beginning with *x*. The excluding of words containing these letters from his material did away with the main difficulty connected with the auditory mode of presentation. Henmon claims he was investigating the relative efficiency of the different modes of presentation, but by omitting words containing such letters as *c*, *q*, *k*, he eliminated one of the difficulties necessarily connected with the auditory mode of presentation. The auditory mode of presentation, therefore, was given an advantage which was not given to the other modes of presentation, since many letters like *e*, *a*, *b*, *l*, look alike in the visual presentation, and may give rise to an indefinite visual perception.

Pohlmann (*op. cit.*) found that auditory presentation is better than visual for young children. This result can be explained by the fact that children are more familiar with spoken language than with written language. Pohlmann also found that auditory presentation is not efficient for the learning of nonsense-syllables because the learner is often uncertain as to what the exact sound is.

Abbott (*op. cit.*) also confirms our findings, for her *O*'s recognized only about one-half of the words when they were pronounced to them.

than a definite visual perception, especially with nonsense material, hence greater attention is required when the material is presented in auditory fashion. Nevertheless in spite of increased attention auditory presentation does not increase the efficiency of learning. To a large extent this heightened attention is aroused by the inherent indistinctness of the auditory percept, and the *O*'s alertness is expended in decisions about the material and not in further impression of the material.

In both the immediate and delayed recalls the material is recalled always as individual words, though the words may have been grouped in the learning. Words which were grouped in the learning came to mind more quickly in the recall one after the other and with a slightly longer pause after the last one, than do words which were not grouped. Those *O*'s who employ a visual schema often in grouping visualize a part of this schema, equal to that which the number of words in the group would require if they were printed in the same fashion as the material used in the visual presentation. The words themselves then come to consciousness, one at a time, usually in visual imagery. Words not grouped in the learning come to consciousness in the recalls, one at a time, but much more slowly than the grouped words.

In many cases an *O* is subjectively certain that the recalled material is correct, but the structure of this subjective assurance is not the same for all *O*'s.<sup>26</sup> The following items, arranged in order of importance, may contribute to this state of consciousness, although not more than two or three of them need be present at any one time. (1) The *O* after recalling a word was able to attempt the recall of the subsequent word without the first word reappearing during the attempted recall of the second word; (2) the imagery of recall comes rapidly to consciousness; (3) the recalled words are pronounced with positiveness; (4) the affective tone is pleasant.

When the words do not come to consciousness quickly either in the anticipating or in the recall, all *O*'s, regardless of their imaginal type, usually recall the words in vocimotor imagery.<sup>27</sup>

<sup>26</sup> Finzi (*op. cit.*) noted that subjective assurance depends upon the distinctness of the memory image; the more distinct the image the more convinced is one of its fidelity. He also found that certain organic sensations are present when the learner is subjectively certain that his recall is correct. Frankfurter and Thiele (*op. cit.*) pointed out that an important requisite for this state is the presence of a spatial schema for localizing the material. Kühn, A. (Ueber Einprägung durch Lesen und durch Rezitieren, *Zsch. f. Psychol.*, 68, 1914, 396-482) noted the following factors as making up the consciousness of subjective assurance: (1) clearness of the visual image, (2) number of helps, and (3) smoothness of recall. Meyer (Bereitschaft und Wiedererkennen, *Zsch. f. Psychol.*, 70, 1914, 161-211) emphasized two factors: (1) quickness of reaction time and (2) definiteness of localization. Pederson (Experimentelle Untersuchung der visuellen und akustischen Erinnerungsbilder, angestellt an Schulkindern, *Arch. f. d. ges. Psychol.*, 4, 1905, 520-534) held that (1) a good perception of the material and (2) a highly concentrated attention were the requisites for a consciousness of subjective assurance.

<sup>27</sup> Von Sybel (*op. cit.*) also found this to be the case. He reported that his learners used more vocimotor imagery and less visual imagery when the series was difficult to acquire.

A schema of localization is usually employed by *O*'s in the recall, especially when the material to be recalled does not come to consciousness quickly. There are three types of schemata: (1) a visual schema; (2) a kinaesthetic schema; (3) a rhythmic schema.

(1) *Visual Schema*. This schema is employed mostly by *O*'s of the visual type,<sup>28</sup> and to a less extent by some *O*'s of the motor type. It consists of a visual image of a sheet of paper with words printed on it. It is usually localized directly in front of the *O* with the words appearing in a vertical column, the first word at the top. In one instance this schema appeared in a horizontal plane with the words running from left to right. When a word does not come to consciousness quickly, the schema appears first. The part of the schema to which the word belongs is most focal, and the rest of the schema is present in a very indefinite non-focal fashion. The word desired then appears in consciousness in visual imagery, usually localized in its proper place in the schema. Some *O*'s are conscious of eye-movement up and down the series localizing the words in the visual schema.

(2) *Kinaesthetic Schema*. This schema is used by the *O*'s of the motor type who employ very little visual imagery. It consists of a kinaesthetic movement or imagery of movement of the hand or head pointing to that part of the series to which the particular word belongs. The extreme left of the *O* represents the first word of the series and the extreme right the last of the series. No visual imagery is present, although there is the kinaesthesia of eye-movement accompanying the manumotor or arm-motor imagery of pointing.

(3) *Rhythmic Schema*. This schema is used by *O*'s of all types, and consists of a vocimotor consciousness of rhythmic sounds. In some instances no definite words or syllables are present.<sup>29</sup> The rhythm, up to the point where the word fails to appear, is repeated by the *O* until the required word is obtained or until the learner gives up his attempt to recall the word.

When the words do not appear in consciousness some *O*'s often set up an *Aufgabe*, usually in vocimotor-auditory terms, to go through the alphabet; i.e., start with *a* and pronounce each letter, expecting that when the correct initial letter is pronounced the word itself will come to consciousness.

The clearness and definiteness with which any schema appears in consciousness is in direct relationship to the difficulty with which the words appeared. If the words come to consciousness after a short pause, the schema, if it appears at all, is non-focal; if the word or group does not appear until a relatively long time has elapsed, the schema is focal and definite in consciousness. Since the schema is more focally present when the recall of words is more difficult, it is therefore more focal in delayed recall than in immediate recall.

<sup>28</sup> Kuhlmann, F. (On the analysis of the memory consciousness, *Psychol. Rev.*, 13, 1906, 316-348) reports that the schema of localization generally precedes the recalling of the material.

<sup>29</sup> Müller (*Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, *Zsch. f. Psych.*, Ergbd. V., 1911, xiv+403) emphasized the importance of rhythm, especially in the first stages of the learning. Müller and Schumann (*op. cit.*) also found that rhythm was a very important factor in learning inasmuch as the syllables which had once formed a part of a metrical foot tended to be associated more closely than syllables not bound together in this way.



The *O* is apt to be uncertain in recall when a word is recalled with difficulty.<sup>30</sup> Two or more of the following factors, listed in order of their importance, usually constitute this consciousness of uncertainty. (1) The first syllable in an associated pair keeps repeating itself; even after the recall of the second syllable the first continues to recur in consciousness. (2) Images of the two syllables alternate or rival in consciousness. (3) A word fluctuates in its position in the schema of localization. There is (4) hesitancy in vocalization, (5) a questioning intonation in vocalization, (6) an unpleasant affective tone.

If in the learning an *O* forms a group which is recalled in motor terms, a motor trend sometimes appears as an aid in recall: as soon as a learner vocalizes one word of a group he 'finds' his vocal apparatus automatically set to say the next word. The group thus becomes a 'motor unit' which runs its course automatically once it is initiated.

In anticipating and recalling the words of a series, the visual imagery of a word may appear (1) typewritten, (2) in the *O*'s own handwriting, or (3) in a form that can not be recognized as any specific writing or printing. There is no conclusive evidence that explains the occurrence of one of these forms rather than another. Most of the visual imagery is of the typewritten form and is derived doubtless from the presented material. It is when the *O* is required to write the words in the learning or in the recall that he has many visual images of his own handwriting. Especially does he seem to visualize his handwriting if he has a characteristic way of forming certain letters.

The *O* of visual type, when presented with material in either auditory or visual fashion, always recalls in visual imagery.<sup>31</sup> The *O* of motor type, when presented with material in visual or auditory fashion, almost always recalls in vocimotor imagery. It appears, therefore, that, regardless of mode of presentation, an *O* recalls material predominantly in imagery of his own type, although supplemented at times by the imagery corresponding to the mode of presentation. Certainly the mode of presentation is in no way indicative of the modality of imagery that an *O* will employ in recalling that material.

When material is recalled with difficulty, the imagery may first appear in the *O*'s own type, and then be completed by imagery corresponding to the imagery of the mode of presentation; or the difficult word may first appear incomplete and unclear in the imagery of the mode of presentation, and then be completed by the imagery of the learner's type. Observers of all types in recalling in auditory terms a material presented auditorily by *E* usually have imagery of their own

<sup>30</sup> Kuhlmann (*op. cit.*) and von Wartensleben (Ueber den Einfluss der Zwischenzeit auf die Reproduktion gelesener Buchstaben, *Zsch. f. Psychol.*, 64, 1913, 321-385) found subjective uncertainty when there was rivalry between two images for the center of consciousness. Meyer (*op. cit.*) found that the greater the lack of subjective assurance the longer the reaction-time, and that indefinite localization conditioned subjective uncertainty.

<sup>31</sup> Abbott (*op. cit.*) points out that a visual image is invariably substituted at once for the heard letters. Frankfurter and Thiele, Meumann and many other investigators have shown that the image of the reproduced word is primarily determined in the ideational type of the learner, and is influenced only secondarily by the mode of presentation.

voices rather than of *E*'s. Occasionally, however, if *E* pronounces a word in a manner which seems odd to the *O*, the recall appears in auditory imagery of *E*'s voice.

In no instance, either in learning or in recall, did any *O* report the presence of a manumotor image.<sup>32</sup> It has been assumed that learning is more efficient if the material to be acquired is written during the act of learning, especially when the material is dictated to the learner as it may be in learning to spell. In the light of our data it appears that the writing-movement, the motor sensations *per se*, do not help at all in the learning.<sup>33</sup> The help comes rather from the *O*'s seeing what he has written, and it is this visual percept only that helps the learner. The act of writing is important and necessary because it makes possible this visual percept and because attention is thus retained longer upon the word than is the case when the word is written by another person.

If the series is difficult to learn and the *O* requires a great number of presentations to learn it, the delayed recall is apt to be relatively poor. This situation arises because successive presentations yield diminishing returns. In most of the later presentations the *O* passes over the words which he has already anticipated; he assumes that they are learned and tries neither to perceive them clearly nor to anticipate them. Thus the later presentations do little to strengthen the associations of many of the words, which after a time pass below the associative threshold as readily as if the final presentations had not been made.

Meumann's rule is that the greater the number of presentations, the greater the strength of the association, everything else being equal. But in no series unfortunately are all things equal. Even nonsense syllables are of unequal difficulty, and the various positions within the presented series are variously favorable. Hence in learning a series it is inevitably true that some parts are learned first and that the final presentations are of greatly diminished value in the further impression of these parts.

<sup>32</sup> One observer, *T*, *Am*, reported with great uncertainty what he thought was a manumotor image, but at no other time was a manumotor image reported by any *O*.

<sup>33</sup> Smith, T. L. (On muscular memory, *Amer. J. Psychol.*, 7, 1896, 453-490) found that errors in recall were reduced 16% by allowing the learner to write the letters; but his subjects were deaf mutes and employed the deaf-mute alphabet. The nature of this material lends itself more readily to motor reproduction than does printed or spoken language. Lay (*op. cit.*) concluded that the writing *per se* is an aid to learning; Fuchs and Haggenmüller and Itschner (*op. cit.*), who obtained results similar to Lay's, explained their findings differently. Fuchs and Haggenmüller pointed out that the material is seen twice when it is written, once when it is presented and a second time after being written. Itschner calls attention to the fact that the presentation-time is longer for the series when the *O*'s write the words and that writing the words destroys an illusion that the material is learned, which occurs prematurely in visual presentation.

R. Dodge (*Die motorischen Wortvorstellungen*, 1896, 78 pp.), although of extreme motor type, never had a manumotor image.



*Objective Data*

In securing the objective data we arbitrarily determined to make the following deductions for each of the possible errors in recall. The deductions are in arbitrary units based on the assumption that perfect recall involves 1,440 units.

	Three-letter syllables	Four-letter words
For a wrong letter.....	32	24
For an omitted letter.....	24	18
For a misplaced letter.....	16	12
For each letter of an unplaced word.....	8	6
For each letter of an interchanged word.	4	3
For an interchanged letter.....	4	3

The deductions were determined in the following manner. The correct recall of a single letter was assumed to count 6 points, and its omission in recall to necessitate a deduction of 6 points from the maximal score. For giving a wrong letter 8 points were deducted, since it is a greater error to give a wrong letter than to give none at all; in this case not only is the correct letter forgotten, but the *lacuna* is also filled in by false data. Since, when a wrong letter is given, more than the total value of a letter is deducted ( $8/6$ ), it is theoretically possible to obtain a negative score if more than three-fourths ( $6/8$ ) is positively wrong in recall. Such a situation, however, if it occurred, would indicate a positive tendency for mislearning and not merely a failure to learn, and should properly be represented by a negative value. For each letter of a misplaced word or syllable, 4 points were to be deducted; for each letter of an unplaced (unlocalized within the series) word or syllable, 2 points; for each letter of interchanged words, 2 points. In the foregoing table of deductions these points have been multiplied by 4 for three-letter non-sense-syllables and by 3 for the four-letter meaningful words, in order that the total values of the two materials might be the same. Thus the score-value of 60 three-letter syllables is  $60 \times 3 \times 4 = 1440$ ; and the score-value of 60 four-letter words is  $60 \times 4 \times 3 = 1440$ . The 1440 units represent perfect recall; deductions are made from 1440 in accordance with the foregoing table, and the remainder is expressed as a percentage of perfect recall (1440 points).

TABLE I

RANK ORDER OF EFFICIENCY FOR EACH MODE OF PRESENTATION. V = VISUAL, A = AUDITORY, M = VOCIMOTOR, m = MANUMOTOR. MODES OF SAME RANK ARE CONNECTED BY SIGN OF EQUALITY

Obs.	Material:	RANK-ORDER											
		1	2	3	4	5	6	7	8	9	10	11	12
C	Meaningful	AMm	VAMm	Vm	A =	Am	VMm =	V	VAm	VAM	VA	AM	VM
	Nonsense	V	VMm	Am	VAMm	VA	VM	AM	AMm	VAM	A	Vm	
Fe	Meaningful	VM = VA	= VAM	VAM	VAm	Am	V	A	VAMm	Vm	VMm	AM	AMm
	Nonsense	VM	AMm	VAMm = A	VAMm = A	VMm	Am	AM	VA	Vm	V	VAM	
S	Meaningful	VM	AM	Vm	A	VA	VAMm	VAM	Am	AMm	V	VAm	VMm
	Nonsense	VM = AMm	VMm	VMm	AM	V	VA	Am	Vm	A	VAM		
W	Meaningful	VM	V	A	Vm	VAMm =	Am	AMm	VAM	VMm	AM	VAm	VA
	Nonsense	VAM	Vm = AM	AM	VA =	V	A	VAMm	VMm	VM	AMm	Am	
Fi	Nonsense	VM	VAM = AM	AM	VMm	VA	A	Vm	V =	Am =	AMm	VAMm	
P	Meaningful	VM	AM	VAMm	Vm	V	VAm	Am	VMm	VAM	AMm	VA	A
T	Nonsense	VAM	VM	VMm	AMm	VA	V	AM	Vm	VAMm	A	Am	
All	Meaningful	VM	Vm	VAMm = V	V =	Am	A	VAM	AM	VAm	AMm	VA	VMm
	Nonsense	VM	VMm	VAMm = AMm	VAMm = AMm	VA	V	AM	VAM	Vm	Am	A	

Table I shows that there is no one mode of presentation which is the best for all *O*'s. Visual-vocimotor presentation, VM, is the most efficient in 7 cases: it is second in efficiency once, fifth once, ninth once, and last once. Nor is the same mode of presentation best for the same *O* with different materials. *W*, for example, finds visual-vocimotor presentation, VM, the most efficient mode in learning meaningful words, whereas it is only eighth best for him in the learning of nonsense material.

TABLE II

AVERAGE EFFICIENCY FOR EACH MODE OF PRESENTATION AS MEASURED BY AVERAGE NUMBER OF PRESENTATIONS REQUIRED FOR LEARNING; ALL OBSERVERS. V = VISUAL, A = AUDITORY, M = VOCIMOTOR, m = MANUMOTOR. LAST COLUMN ARRANGED IN ORDER OF EFFICIENCY WITH LEAST EFFICIENT MODE AT THE TOP

Order of Presentation	Mode Presentation	Meaningful Words	Nonsense Syllables	Average Two Materials
7	Am	5.90	7.83	6.87
3	VA	8.10	5.58	6.84
10	AMm	7.80	5.92	6.86
2	A	6.00	7.33	6.67
11	VMm	9.10	4.17	6.64
8	VAM	7.00	5.83	6.42
5	Vm	4.80	7.58	6.19
6	AM	7.20	5.00	6.10
12	VAMm	4.00	6.90	5.85
1	V	5.90	5.25	5.58
4	VM	3.40	3.42	3.41
9	VAm	8.00	....	....

Table II again shows that the combined visual-vocimotor mode of presentation is by far the most efficient. The amount of variation between the other modes of presentation is so slight that no significant differences are apparent.

These objective data do not properly afford an answer to the problem of the most efficient mode of learning, because in the first place the determinations are too few to allow of a significant statistical treatment, and because the general averages fail to take account of the imaginal type of the *O*'s. If statistics are to tell the true story, an average must represent not a single mode of presentation but a single mode of learning. The learning-process must be introspectively controlled or at least viewed in the light of the previously determined type of the learner, and averages must be found for similar modes of learning, even though they occur with dissimilar modes of presentation.

## SUMMARY AND CONCLUSIONS

1. The process of learning consists of three distinct stages: (a) the orienting stage (pp. 269 ff.); (b) the stage of attempted anticipation (pp. 271 f.); and (c) the anticipatory stage (pp. 272 ff.).

2. All of our *O*'s found it necessary to employ vocimotor imagery in learning a series of meaningful words or nonsense-syllables; no *O* was able to learn a series of words or nonsense-syllables if he succeeded under instruction in inhibiting vocimotor imagery from the start. The *O*'s of visual type in most cases did not employ vocimotor imagery after the first few presentations of the material in the learning, but the vocimotor imagery was present during the initial presentations and did not lapse until the visual imagery had become clear and definite. The *O*'s of the motor type are never able to inhibit vocimotor imagery and yet learn the material.

3. If the *O* is instructed not to use vocimotor imagery during learning he responds (a) by attempting actively to inhibit vocimotor imagery, thus interfering with or preventing learning, or (b) by setting up for himself a new *Aufgabe* to use imagery of another modality (e.g., visual imagery).

4. The *O* in learning usually groups the words or syllables; and the number of words or syllables which constitute a group depends upon (a) the time intervening between the presentation of two successive words, (b) the immediate memory-span of the *O*, (c) the meaningful associations between the words or syllables, (d) the presence of an *Aufgabe* for grouping that the learner himself may set up, and (e) the position of difficult words within the series.

5. Manumotor imagery does not aid either the learning or the recall. When the material to be learned is presented to the *O* in auditory fashion, the learning is in most cases more efficient if the *O* is required to write the material than if he does not write it; and the increased efficiency occurs especially when the materials are isolated words or syllables or when auditory perception is less definite than visual perception. In these cases the *O* must decide upon a definite spelling in order to write the word pronounced to him, and the writing thus definitizes the perception. The advantage of writing, therefore, comes not from manumotor processes but from the visual percept of the written word.

6. The mode of presentation is in no way indicative of the modality of imagery that an *O* will employ in learning or

recalling a series of words or syllables. The modality of the imagery which a learner employs is determined primarily by his ideational type and only secondarily by the mode of presentation.

7. All *O*'s find it difficult to obtain a definite auditory perception of some syllables, especially those syllables containing the letters *c, q, k; d, t; etc.*

8. The recall of difficult words or syllables is in most cases preceded by the appearance in consciousness of a schema in one of three types: (*a*) visual schema (p. 275), (*b*) kinaesthetic schema (p. 275), (*c*) rhythmic schema (pp. 263, 275).

9. In addition to the words themselves and their associations there may occur as an aid a 'motor trend.' This 'motor trend' is present in the acts of anticipation but more frequently in the recalls.

10. Extraneous associations with the words to be learned are formed by all the *O*'s. There seems to be no well-defined chronological order in which the words and these associations appear in recall. The word appears sometimes in the imagery corresponding to the learner's ideational type and sometimes in the imagery corresponding to the mode of presentation; and the association similarly may come either in the imagery of the learner's ideational type or of the mode of presentation.

11. Words which have these extraneous associations are in most instances retained better than words not thus associated.

12. The first two or three and the last two or three words of a series are the first words of the series to be learned.

13. Visual imagery is not efficient for a rapid anticipation and usually gives way to vocimotor imagery when rapid anticipation is acquired.

14. In visual-auditory presentation the learner seldom attends equally to both the auditory and the visual aspects of the presentation. He attends usually almost wholly to the one or to the other according to his ideational type.

15. The recall of a series is sometimes accompanied by subjective assurance and sometimes by subjective non-assurance.

16. The statistical data obtained in this study are significant in scarcely a single instance for the reason that, although the objective conditions were kept constant in accordance with the rules for such investigations, the subjective factors could



not be brought under control. At best mere objective data will do little more than indicate the most efficient mode of presentation for a particular *O*, until account is taken of the ideational type of the *O*'s, the attentive selection that they exercise among the various presentative aspects of a material, and the manner in which one sensory mode is subject to translation into another. Some introspective procedure is a necessity.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF  
VASSAR COLLEGE.

XLII. VOLUNTARY CONTROL OF LIKES AND DISLIKES; THE EFFECTS OF AN  
ATTEMPT VOLUNTARILY TO CHANGE THE AFFECTIVE VALUE  
OF COLORS.

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By MARGARET FLOY WASHBURN AND SARAH LOUISE GROSE.

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Marcus Aurelius said: "Everything is opinion, and opinion is in our power." It is the second part of this comfortable aphorism that most arouses our distrust. When we test it critically, it seems to resolve itself into the much less satisfactory statement that one of our opinions is in the power of another of our opinions when the two come into conflict; the most encouraging feature of the case is that the victory in such a conflict may through the operation of certain mental processes be conferred on that opinion which seemed at the outset weaker. Such a conflict is especially vital when the opinions are not merely intellectual but emotional; when they are desires. Practical life often confronts us with the advisability of 'changing our desires rather than the order of the world,' to use another Stoic phrase; of liking the things we at first disliked, and regarding with aversion the things that were originally attractive to us. This feat can actually, by a person with normal mental inhibitions, be performed; and one of the most important problems in practical psychology concerns the methods by which it can be performed.

If we reflect on the process as it occurs in experience, we find that, in the first place, the nature of our organism itself has furnished us with a powerful aid in the overcoming of desires. Through the law of affective fatigue or adaptation an emotion tends naturally to pass over into its opposite; violent delights have violent ends. This natural tendency, which operates on good and bad desires alike, may be utilized by the individual who wishes to overcome a desire, and may probably be somewhat accelerated in its action by the other methods which we will proceed to mention.

A second method by which our affective attitude to certain stimuli may be changed is that of fixing attention on different elements in the stimulus. The object desired is usually sufficiently complex to be not all desirable or all undesirable. One who wishes to change his affective attitude towards a given object may turn his attention from its merits to its defects, or *vice versa*, provided of course that the existing emotional reaction is not so strong as to make attention immovable.

Thirdly, we can alter our likes and dislikes by transforming reality through the aid of imagination. That is, the object may be surrounded by a set of ideas unlike its actual setting, and thus become much more or much less desirable. A person who wishes to overcome a passion for another may imagine that other placed in different social conditions, where his or her defects of character or training would be

more apparent, or may ask himself how the individual in question would 'wear' as an unescapable daily companion. This device, in proportion as the imaginary circumstances are improbable, if it succeeds in altering the emotional attitude, does so by virtue of the mechanism which the Freudians have called phantasy; a form of withdrawal from reality.

Finally, it is possible to change the affective attitude by deliberately performing the movements and reactions which belong to the opposite attitude, so far as these reactions are controllable. This is the Freudian mechanism of compensation: whistling to keep up one's courage; declaring in words and acts as strongly as possible that one loves the thing one really hates, or did hate in the beginning.

We may call these four devices by the following terms respectively: (1) affective fatigue; (2) shift of attention; (3) imaginary context; (4) compensation.

The present study undertook to observe the process of deliberately altering the affective reaction to color, presented in the form of small pieces, 3 centimeters square, pasted each in the middle of a card  $2\frac{1}{2}$  by 3 inches. One of these cards was laid on a table before the observer, who was asked to express her judgment of the pleasantness of the color by using one of the numbers from 1 to 7, 1 meaning 'very unpleasant,' 7 'very pleasant,' and 4 'indifferent.' When the judgment had been expressed, the experimenter said "Now I want you to see if you can dislike that color," if the judgment had been favorable; or, "I want you to see if you can like that color," if the judgment had been unfavorable. The original judgment and the altered judgment were recorded, and the observer was then asked how she had effected the change. This proceeding was followed with each of eighteen Bradley colors, namely, saturated red, orange, yellow, green, blue, violet and the lightest tint and darkest shade of each of these six. About two months later each observer was asked to judge the pleasantness of the colors again; this was done in order to study the permanence of the changes voluntarily produced.

The total number of observers from whom complete returns were obtained was 53; since each judged eighteen colors, there were in all 954 experiments in each of the two series, the earlier and the deferred series.

In 6.3% of these experiments, the observer was unable to change her judgment of the color.

Of all the 1 (very unpleasant)	judgments made, 14% could not be changed.
" " 2 (moderately unpleasant)	" " 5.7% " " " "
" " 3 (slightly unpleasant)	" " 3.2% " " " "
" " 4 (indifferent)	" " 1.9% " " " "
" " 5 (slightly pleasant)	" " 5.4% " " " "
" " 6 (moderately pleasant)	" " 6.7% " " " "
" " 7 (very pleasant)	" " 8.8% " " " "

As was to be expected, it was thus found that *extreme judgments are harder to change voluntarily than moderate judgments*. More interesting is the result that *judgments of extreme unpleasantness are decidedly harder to change than judgments of extreme pleasantness*.

Where the effort to change the judgment was successful, we may note certain points with regard to the extent of the change produced. Evidently the judgments 1 and 7 had equal chances to be altered by six points. The results show that in 9% of the 1 judgments the effort to find the colors more agreeable resulted in raising the judgment to 7; while in 32% of the 7 judgments the effort to find the colors less

agreeable altered the judgment to 1. It seems clear that under our conditions it is *much easier voluntarily to lessen the pleasantness of a color by six points than voluntarily to raise its pleasantness by six points.*

A similar relation, though less marked, holds between raising and lowering pleasantness by five points. The judgments 2 and 6 have equal chances to be changed five points. Of the 2 judgments, 36% were by the effort to find the colors more pleasant raised to 7; of the 6 judgments, 48% were by the effort to find the colors less pleasant lowered to 1. *It is noticeably easier voluntarily to lessen the pleasantness of a color by five points than voluntarily to raise its pleasantness by five points.*

In a number of cases where the effort to change the affective value of a color succeeded, the original judgment recurred after the two months' interval; that is, the pleasantness of the color was estimated after the interval just as it had been estimated when first shown, before the effort was made to alter it. Of the 7 judgments, 29% thus recurred; of the 6 judgments, 26.9%; of the 5 judgments, 28.3%; of the 4 judgments, 25.7%; of the 3 judgments, 18%; of the 2 judgments, 25%; of the 1 judgments, 26.7%. It thus appears that *pleasant judgments, though more easily changed than unpleasant judgments, have a somewhat greater tendency to recur later.* The average percent. of recurrence for pleasant judgments is 28; for the unpleasant judgments it is 23.2. Part of this difference is caused by the very low tendency of the judgment 3 to recur; a fact for which we have no explanation to suggest.

In a number of other cases the judgment made after the two months' interval coincided not with the original judgment, but with the judgment as altered by the observer's effort. Here it is interesting to observe the relation between the amount and direction of the change effected, and its permanency.

Of the changes where pleasantness was raised	1 point,	24%	were permanent.
" " " " " " lowered	" "	32%	" "
" " " " " " raised	2 points,	16%	" "
" " " " " " lowered	" "	8%	" "
" " " " " " raised	3 "	14%	" "
" " " " " " lowered	" "	12%	" "
" " " " " " raised	4 "	11.8%	" "
" " " " " " lowered	" "	1.6%	" "
" " " " " " raised	5 "	2.6%	" "
" " " " " " lowered	" "	2.3%	" "

Obviously accidental variation would often be responsible for slight changes from the original judgment, so one would expect that one point changes would have the greatest tendency to recur. Where the amount of change was more than one point, it is noticeable that *changes in the direction of increased pleasantness are more likely to be lasting* (av. per cent of permanence of former, 11.1, of latter, 5.9).

On the whole, these results might be interpreted as suggesting the existence of a *superficial pessimism operating on a deeper-lying optimism.* The pessimism is indicated by the fact that it is easier to pass from strong liking to strong disliking than to go in the reverse direction, and by the fact that it is harder to change at all a judgment of extreme disliking than a judgment of extreme liking. *At the moment, our observers were more ready to abandon their likes*



than their dislikes. But in the long run, the tendency was towards optimism; the observers inclined to recur to their original likes more than to their original dislikes, and to be more lastingly influenced by the favorable changes which they had effected in their reactions to the colors than by the unfavorable changes.

We may next consider the methods which they used to bring about the changes. *By far the most frequently used method was the one which we have called 'imaginary context.'* The color was thought of in an imaginary setting different from the actual one. In 32% of the entire 954 experiments, altering the context produced a change in the affective judgment. For example, the shade of red was judged 5 at first; imagining it in wall paper lowered its pleasantness to 2. Where this method of altering the affective judgments was used, it produced marked changes. Of the changes amounting to 6 points, 50% were due to altered context; of those amounting to five points, 46%; of those amounting to four points, 40%; of those amounting to three points, 39%; of those amounting to two points, 30%; of those amounting to one point 24%. It was a little more effective in raising than in lowering the affective values, but the difference was not marked (average, 40% of raising, 36% of lowering).

A particular form of altered context, applicable only to color sensations, is *the imagining the color in combination with some other color.* In 17.7% of the 954 experiments, the affective value of the colors was changed by this method. It was less effective in producing six point changes than other forms of altered context: 21% of the six point changes were due to this cause, 29% of the five point changes; 20% of the four point changes, 22% of the three point changes, 20% of the two point changes, and 5.9% of the one point changes. It was somewhat more effective in lowering the affective value of the colors than in raising it (average, 20.6 of raising, 26.9% of lowering).

Still another form of altered context consists in imagining a greater or less amount of the color. In 4.2% of the total number of experiments the change of affective value was due to imagining the color in greater quantity; in only one of these 41 cases was the effect thus produced that of raising affective value. *Imagining the quantity of a color to be increased nearly always lowers its pleasantness.* This influence is most effective in producing slight changes of affective value; it caused no six point changes, 4.8% of the five point changes, 12.8% of the four point changes, 9.6% of the three point changes, 10% of the two point changes, 18.7% of the one point changes. In only two percent of the total number of experiments was the affective value of a color altered by imagining less of it; obviously because of the small size of the bits of color shown. The effect of lessening the amount of the color in imagination was only in one case an increase of the affective value, the change then amounting to one point. The greatest effectiveness of this device was in raising the pleasantness two points, 10% of the cases of such raising being due to its influence.

The effect of shift of attention, the voluntary direction of attention to some disagreeable aspect of the color, was shown almost entirely through a *direction of attention to some agreeable or disagreeable association* with the color. Although this involves the use of elements not actually presented in the color itself, it cannot be said to constitute



a transformation of reality in the sense that altered context does. It is a recollection of actual experience. Recall of associations with the color was the controlling influence in 14.1% of all the experiments, and had its maximum effectiveness in the case of large changes in affective value, and it was *markedly more influential in raising than in lowering pleasantness*. Of the six point changes towards pleasantness, 33% were due to associations; of the six point changes towards unpleasantness, 33%; of the five point changes toward pleasantness, 15.8%; of the five point changes toward unpleasantness, 7%; of the four point changes toward pleasantness, 31%; of the four point changes toward unpleasantness, 11.2%; of the three point changes toward pleasantness, 25%; of the three point changes toward unpleasantness, 18%; of the two point changes toward pleasantness, 25%; of the two point changes toward unpleasantness, 10%; of the one point changes toward pleasantness, 17%; of the one point changes toward unpleasantness, 12.5%. The average percentage for increasing pleasantness was thus 24.4; for decreasing pleasantness, 15.3. This fact, that it is *easier to recall pleasant than unpleasant associations with colors*, is in accord with a point established previously<sup>1</sup> in our laboratory, namely, that when the affective value of a color changes spontaneously during fixation for one minute, increase of its pleasantness is more likely to be due to association than is decrease of its pleasantness.

The colors, being such simple objects, did not in themselves present enough variety of aspect to allow the shift of attention from agreeable to disagreeable features or the reverse. The nearest approach to such a process was shown in the case of 17.6% of the one point drops in pleasantness, which the observers ascribed to finding colors 'insipid.' It is hard to decide whether these cases do not come under the next head, that of *affective adaptation*.

This influence was exercised in 2.2% of all the experiments. It had *very little power to produce marked changes* in judgments of affective value, and what power it had in connection with such changes was to *lower pleasantness*. In 2.3% of the six point drops in affective value it was the cause at work, and in 1.17% of the five point drops. Its effectiveness was oftener shown in the case of the *slight changes*, where it brought about *both increased and lowered pleasantness*; of the two point drops it was responsible for 5.6%; of the one point rises, for 13.3%; of the one point drops, for 14.7%. Probably the slight influence of affective adaptation in these experiments is due to the mildness of the emotional reactions involved.

Finally, *true compensation*, the deliberate assumption of the opposite affective attitude, was used by only one of our observers, and by her only three times. She raised the value of one color from 1 to 3 'just by trying suggestion,' and lowered that of two from 6 to 5 by 'concentrating on getting them down.' It is easy to conjecture that *this method*, so useful in ordinary life, would naturally play little part in conditions where the affective state is not only mild, but accompanied merely by simple motor expressions. One may assume

<sup>1</sup> "The tendency of associated ideas is to raise the pleasantness of a color." Washburn and Crawford: Fluctuations in the Affective Value of Colors during Fixation for One Minute. This Journal, 22, 1911, 579-582.

hatred in order to counteract love, for example, because hatred may be expressed by a great variety of movements, by torrents of words, by forcible actions, and when these voluntary movements are set in operation, there is a fair chance that the deeper organic movements associated with them may come into play and the emotion really be transformed. But expressing one's like or dislike of a color is so mild and simple a motor process, that its voluntary performance can have no very profound effect.

## BOOK REVIEWS

*Der Geruch.* By HANS HENNING. Leipzig, Barth, 1916, viii, 533 pp.

Henning's notable contribution to the psychology of smell first appeared in the *Zeitschrift für Psychologie*. Its parts are scattered through various numbers, beginning with the one dated October, 1915 and ending with that dated September, 1916. The appendix of the book contains not only a special study of the sense of smell in ants but also a very important discussion of taste-qualities. Both these papers appeared in the number of the *Zeitschrift* dated February, 1916. The bound volume contains a few additional notes, a name-index and an index to the scents discussed.

In the opinion of the reviewer, Henning's work marks the beginning of a new era in the study of smell and probably also in that of taste. In his treatment of his predecessors, Henning is a ruthless—in fact, a very uncivil—iconoclast. To change the figure, he forgets that if the work of Zwaardemaker, Nagel and Aronsohn had not been done, he would have had to break some quite rocky ground which, as a matter of fact, he has had only to plough over and to plant anew. Nevertheless, one is reluctantly compelled to admit that the authors of our standard text-books will have to rewrite *ex radice* their chapters on smell, chapters based for the most part on the conclusions of Zwaardemaker in his *Physiologie des Geruchs*, published twenty-six years ago. Twenty-six years make more than half the life-time of experimental psychology, and Henning's criticisms not only are searchingly destructive but also carry full conviction. This conviction is half dismaying and half comforting to anyone who has tried faithfully but unsuccessfully to reproduce the results of Zwaardemaker's compensation and exhaustion experiments or to obtain definite evidence for the Linnaean classification of odors—adopted by Zwaardemaker (be it said) not as final but as one feature of an experimental programme.

The number of Henning's experiments and the expertness of his subjects, in which he has the better of all other experimenters in his field, lend great weight to his positive conclusions. The writer confesses that, when she reviewed early in 1916 (in the *Psychological Bulletin*) the first instalment of his work, she was antagonized by his derisive treatment of earlier experimenters. She also confesses to lacking the wide and exact knowledge of biology and organic chemistry which are necessary for a reliable evaluation of some of Henning's most important findings. But from the experimentalist's point of view, she cannot forbear saying again that one looks almost in vain in his pages for precise details of the procedure employed in particular groups of experiments. One canon of scientific investigation Henning certainly violates to some extent; he seldom makes it possible for convert or critic to reproduce the minutiae of his experimental conditions. To this criticism on his work in classification he has replied by saying (p. 360): "Ich finde, zum Experiment genügt eine Flasche mit Riechstoff, die jeder kennt." With this remark anyone who has worked much with olfactometers must have some sympathy.

Another peculiarity, which seems unfortunate in so polemic a writer as Henning, is that he rarely reduces the introspective data obtained from his subjects to statistical form and does not mention, except to explain away, instances in which their reports conflicted with his conclusions. For the most part, his method of procedure is to state his conclusions dogmatically, but amply to illustrate them from the reports of his observers. He says very truly (p. 360) that the critical introspection of trained psychologists [such as he numbered among his observers] is more valuable than statistics taken on all the students in the University, and that the statistical procedure, about which science in America has so far raved (*immer noch krankt*), has by no means the precision of a qualitative analysis. But the point is not that his experiments were deficient in number or faulty in method—the writer's belief is quite to the contrary—but that he gives us little or no inkling of ever obtaining inconvenient data, and that he thus fails to create an impression of taking a dispassionate attitude toward his own work in comparison with that of others. Nevertheless—whatever omissions of matter or faults of manner Henning's book may show—it not only represents an enormous amount of work both in laboratory and in library, but also represents the application of a keen and original mind to a line of research which has long badly needed and really merited more enthusiastic pursuit.

Even though a large part of Henning's work consists in a critical examination of the 'literature,' it is impossible in the space allotted to summarize his book at once completely and clearly. It seems best, therefore, to confine the remainder of this review to indicating in the briefest possible fashion the character of his subjects, material and apparatus, to stressing two or three important points which he makes in regard to method in smell experiments, and to stating intelligibly those of his conclusions which most strikingly contravert the findings of earlier authorities.

His experiments were made at Frankfort on the Main, where he was *Privatdozent* at the time his book was written. He made more or less extended series of individual experiments on thirteen grown persons and three children. He also made certain group-experiments on forty-six university students. His really most expert subject was his wife, a trained psychologist, who conducted experiments in which he himself had the chance to serve as observer. Of the fourteen chief subjects (including Henning), four were psychologists by profession, six in all were trained psychologists, and ten in all had at least an elementary knowledge of psychology. Of the professional psychologists two—Henning himself and Professor Cornelius—were thoroughly grounded in chemistry, the latter being especially well-acquainted with odorous materials. Of the other subjects, one had considerable knowledge of chemistry, and another was familiar with clinical laboratory-work.

In his chief series of experiments Henning used 415 different scents, selected to represent the whole qualitative range of natural odors and including in about equal numbers chemically pure substances (such as essential oils) and natural scents (such as dried herbs). He also submitted the odors of daily life to systematic examination and even made excursions with his subjects to the Zoölogical Garden and to other places rich in smells in order that no natural odor essentially different from the 415 specimens might escape him. He also made some use of fifty-one odorous trade-articles, such as perfumes, official preparations and ink. He kept many of his scents at hand

in five different concentrations. On the average, in his qualitative experiments, about ten smell-exposures were made to a subject at a sitting. For olfactometric purposes he used six different devices, including various forms of Zwaardemaker's olfactometer (which he criticizes severely).

Not the least valuable part of Henning's contribution consists in the following methodological points. First, the quality of a scent cannot be fully appreciated by "monorhinc" smelling, smelling with one nostril only, which is unnatural. From this it follows that mixture of qualities by "dichorhinc" smelling—smelling one with one nostril and another with the other—is unsatisfactory. "Dirhinc" smelling alone gives clear-cut perception. Secondly, reliable judgments with regard to smell-similarities can be obtained only from observers who do not know the nature of the scents with which they are dealing. Henning distinguishes between the true odor (*Gegebenheitsgeruch*), which is obtained by the observer who is smelling with closed eyes and is ignorant of the nature of the scent, and the object-smell (*Gegenstandsgeruch*), which (like color) is projected upon the object from which it is known to come and is apt to be distorted by associative supplementing. Both upon this point and upon that of monorhinc smelling such experience as the present writer possesses is fully in accord with Henning's.

The most revolutionary of Henning's conclusions have to do (1) with the interrelation of smell-qualities, (2) with the phenomena of smell-mixture, (3) with the phenomena of smell-exhaustion, and (4) with the qualities of taste. In the discussion of these conclusions some of his other findings will incidentally appear.

(1) He holds that smells, like colors, constitute a tridimensional manifold, although smell-qualities are related to one another in quite another fashion than are color-qualities. In the first place, the groupings of odors must be represented by a prism rather than by a double pyramid. Of this prism, the triangular faces are equilateral and the rectangular faces are squares. At the angles should stand the most typical smells of his six fundamental classes. At the corners of one triangle should stand respectively the most typical flowery, fruity and putrid smells; at the corners of the other should stand the typical spicy smell on the same edge with the flowery smell on the other triangle, the typical resinous (*harzig*) smell on the same edge with the fruity, and the typical burning (*brenzlich*) smell on the same edge with the putrid. He regards violet as the most typical smell of the flowery class, lemon of the fruity, sulphuretted hydrogen of the putrid, nutmeg of the spicy, frankincense of the resinous, and tar of the burning. Transition-smells lead from every class into every other; the classes which stand diagonally opposite to each other on the square faces of the prism are to be regarded as connected with each other by diagonal lines across these faces. Instances of transition-smells are as follows: (1) between flowery and fruity, geranium and sandalwood; between flowery and putrid, the smell of decaying flowers, and between fruity and putrid, the smell of decaying fruit; between flowery and spicy, thyme and vanilla; between fruity and resinous the various piney odors; between putrid and burning, the ammoniacal animal odors; between flowery and resinous, the smells of the fragrant gums; between fruity and spicy, the mints; between putrid and spicy, garlic; between putrid and resinous, fish-scales; and between burning and all the remaining classes (theoretically at least) the smells obtained by burning scents of these classes. But, in the



second place, odors differ from colors in the fact that likeness between smells does not correspond with an overlapping of the physiological processes concerned. Henning sets his face against specific energies in the realm of smell, against original smells comparable to the *Urfarben* of the color theories. It is only by courtesy that we can say that any natural smell is counterfeited, that the smell of violets, for example, is counterfeited in the perfume industry. The smells at the angles of the prism are indeed related to one another like the colors at the angles of the pyramid, but the smells on any one edge or diagonal are related to one another like the tones in the tonal series. A mixture of two such smells does not result in a simple blend like the orange obtained by mixing red and yellow. In fact, the prism cannot be made to illustrate *both* the transition from one simple smell to another through other simple smells (for example that from lemon to nutmeg through certain minty odors) *and* also the transition from one to the other as produced by mixing the two in varying proportions. For if mixtures are transitional between two smells on the surface of the pyramid, they cannot logically be placed upon a line drawn through its substance. The whole surface of the prism, however, and not merely its edges and diagonals, must be thought of as occupied by simple smells localised according to their two-sided, three-sided or four-sided resemblances. Thus, all simple inorganic scents would stand somewhere on the fruity-putrid, burning-resinous face. The inside of the prism may be vaguely imagined as reserved for unblended mixtures of smells on different faces.

Henning bases his classification primarily on the descriptions given, and the confusions made, by his subjects in extended series of experiments in which they were instructed carefully and without haste to describe the qualities of scents presented. These experiments were supplemented by others in which the observers were given a number of scents and were required to arrange the similar ones in series and to throw out the others. He finds secondary (but probably very real) support in organic chemistry. Assuming that the quality of an odor depends not on the composition but on the pattern of the scent-molecule (which is probably true), he points out a striking sameness of pattern in the benzol rings of the aromatics belonging to any one of his six classes, and also the transitional character of the patterns corresponding to transitional smells. Henning scouts any connection between odor and the periodic system. Odor is as much a constitutive property of the molecule as is fluorescence.

The replacing of the linear or one-dimensional grouping of the Linnaeus-Zwaardemaker classification by a tridimensional grouping appeals strongly to the present writer. An instance of the difficulties which crop up out of the former classification is this: a short-cut apparently exists between the camphor-turpentine and the lemon-rose subgroups (under the "aromatic" group) beside the recognized but longer path through the spices and the mints. Years ago, in personal conversation with the writer, Zwaardemaker noted the resemblance between oil of orange and pure turpentine, and also the difficulty created by the many-phased smell of oil of juniper, which Henning places on the flowery-fruity-resinous-spicy face of his prism.

(2) Henning denies any analogy between the phenomena of color-mixture and those of smell-mixture, which are rather parallel to the phenomena of tonal fusion. When certain smells are mixed, the result is indeed comparable to the most perfectly welded chords, which Stumpf admits are simple in direct sensory experience. The best

instances are such natural blends as one encounters in oil of juniper and such stable blends as one finds in the best artificial perfumes. When other odors are mixed, one may have rivalry or the suppression of one smell (or smells) by another. Compensation, the cancellation of one smell by another, is a myth. Henning experimented on forty-six university students with Zwaardemaker's olfactometer and never obtained a single instance. Alleged cases are explicable by fatigue or by exhaustion in the first inhalations of all the free scent-particles in the olfactometer. The phenomenon certainly never occurs in free air. Another possibility, which is best realized in dichorhnic smelling, is the "coincidence-smell," in which the two odors are held apart by a strain of attention (*Aufmerksamkeitsspannung*) and yet have a certain unitary character. Still another possibility, which of course depends on dichorhnic smelling, is the "duality-smell," in which the two components are clearly localized, the one in the right nostril and the other in the left. Blends which are perfect at the first instant of mixture are not necessarily stable. In a little while one may get the coincidence-smell or the duality smell, or one may succeed by shift of attention in making now one and now another smell stand out on a background made up of the rest. This is the phenomenon of the "successive smell." In general, the more similar the smells are, the more perfect their fusion is; within limits their intensity makes little difference.

(3) Henning holds that the phenomena of smell-exhaustion have been exaggerated. He urges with justice that the nervous apparatus of smell should be no more subject to fatigue than is that of the eye or ear. The terminal apparatus of smell may indeed be subject to fatigue, but strong smells cannot be made to disappear merely by exhaustion. Cases of apparent exhaustion are largely explicable by failure of attention to weak and persistent stimuli. Moreover, the effect of fatigue upon the sensory epithelium cannot as yet be distinguished clearly from the toxic effect, local and general, of long continued smelling. Henning describes in detail the toxic effects, marked and lasting for days, produced on one of his subjects by smelling in quick succession 150 different scents, from which substances known to be poisonous had been excluded. The observation was confirmed by experiences with other subjects. The present writer has made similar observations in the case of subjects (particularly herself) who were memorizing long series of smells. But in her opinion, Henning makes too pathological a matter of smell-exhaustion, so-called, and also exaggerates the rôle played by the failure of "sensory attention" in producing insensitiveness to a scent to which one is long exposed. May not smell-exhaustion be comparable with adaptation in the case of other senses? And may not this adaptation be of peripheral origin?

Henning maintains that when sensitiveness to a given smell is dulled by exhaustion, this dullness exists for *that particular odor only*; although, if attention has weakened, it will be poor also for any very similar odor. Aronsohn's method of attempting to arrive at a physiological classification of smells through the effect of exhaustion by one scent upon sensitiveness to another is absolutely valueless. If Aronsohn (says Henning on p. 267) had known the chemical composition of the scents he was using he would never have published his results, for in some instances in which he declared that he could smell one scent but not another, the odorous principle of the two was exactly the same. The differentiation of different parts of the

olfactory membrane to correspond with different smell-qualities is rendered highly improbable by the patchy and asymmetrical distribution of the membrane in the smell-clefts of the two nostrils. Henning points out, it may be said in passing, that the scent-molecules must penetrate into the epithelium and there quickly suffer a chemical decomposition such as to make them odorless, else every smell would persist indefinitely.

(4) No account of Henning's revolutionary findings would be at all complete without at least a brief notice of his taste-tetrahedron. He holds that sweet, salt, sour and bitter are not the only simple tastes, but stand related to one another as do the colors at the angles of the pyramid and the smells at the angles of the prism. Instances of transition-tastes are these: between salt and sour, bicarbonate of soda; between salt and sweet, the alkaline tastes; between salt and bitter, potassium bromide; between sour and sweet, acetate of lead; between sour and bitter, potassium sulphate; and between sweet and bitter, acetone. The phenomena of taste-mixture are closely comparable to those of smell-mixture.

This review does scant justice to the wealth of material in Henning's book, material which must be of great interest alike to the psychologist, the biologist and the chemist. It should give new impetus to experimenters who have put away the scent-bottle and the olfactometer in despair of reproducing or in any way confirming authoritative results which they have not had Henning's (somewhat unhallowed) courage to reject.

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E. A. MCC. GAMBLE

The following books have also been received:

- W. BRUHN. *Theosophie und Anthroposophie*. Leipzig and Berlin, B. G. Teubner, 1921. Pp. 108.
- J. COHN. *Führende Denker. Vierte Auflage*. Leipzig and Berlin, B. B. Teubner, 1921. Pp. 117.
- B. ERDMANN. *Grundzüge der Reproduktionspsychologie*. Berlin and Leipzig, W. de Gruyter & Co., 1920. Pp. viii., 186. \$1.80.
- S. FREUD. *Drei Abhandlungen zur Sexualtheorie. Vierte Auflage*. Leipzig and Wien, F. Deuticke, 1920. Pp. vii., 104.
- J. L. DES BANCELS. *Introduction à la psychologie: l'instinct et l'émotion*. Paris, Payot & Cie., 1921. Pp. 286.
- D. I. BUSHNELL, JR. *Native Cemeteries and Forms of Burial East of the Mississippi*. Bureau of American Ethnology, Bulletin 71; Washington, D. C., Government Printing Office, 1920. Pp. 160.
- A. GEMELLI. *Le dottrine moderne della delinquenza*. Milan, Società Editrice 'Vita e Pensiero,' 1920. Pp. xvi., 212.
- C. READ.—*The Origin of Man and of his Superstitions*. Cambridge, The University Press, 1920. Pp. xii., 350.
- M. HAMBURGER. *Vom Organismus der Sprache und von der Sprache des Dichters*. Leipzig, F. Meiner, 1920. Pp. vii., 189.
- C. PLATT. *The Psychology of Thought and Feeling*. New York, Dodd, Mead & Co., 1921. Pp. x., 290.
- C. BAUDOUIN. *Suggestion and Autosuggestion*. Translated by E. and C. Paul. New York, Dodd Mead & Co., 1921. Pp. 349.
- L. VIVANTI. *Principii di Etica*. Rome, P. Maglione & C. Strini, 1920. Pp. viii., 314.
- A. S. EDWARDS. *The Fundamental Principles of Learning and Study*. Baltimore, Warwick & York, 1920. Pp. 239.



## BOOK NOTES

### PSYCHOANALYSIS IN 1920, BY G. STANLEY HALL

*The International Journal of Psychoanalysis.* Directed by SIGMUND FREUD. Edited by ERNEST JONES. London, Internat. Psychoanalytic Press, 1920. Vol. I, Part I, 124 pp.; Pt. II, 97 pp.

The most important recent publication in the field of psychoanalysis is the above journal, which appears at a time when, owing to the presumed disturbed economic and political condition of Austria, the *Zeitschrift*, *Imago*, and *Jahrbuch* show signs of languishing in quality, quantity, and frequency of appearance. And as Freud himself appears as co-editor and contributor, and especially as England, since the appearance of Trotter's publication, has such a galaxy of able and original devotees of the cult, the center of the movement may henceforth be gradually transferred to London. This journal will be a godsend to all English readers interested in this cult if it can maintain the high level on which it has been begun.

The first two parts contain an excellent review of J. J. Putnam's contributions, and two articles by Freud, in one of which he amplifies the thesis that there have been three great scientific movements: the first marked by Copernicus; the second by Darwin; and the third by the discovery and exploitation of the unconscious. There are very good reviews of literature, especially of Jones' "Recent Advances in Psychoanalysis;" accounts of the proceedings of societies, etc. The journal is well printed and so well edited that it cannot fail to be a helpful and stimulating competitor not only of the *Zeitschrift* but also of our own excellent *Psychoanalytic Review*.

*A General Introduction to Psychoanalysis.* By SIGMUND FREUD. N. Y., Boni and Liveright, 1920. 406 pp.

Freud here, in a course of twenty-eight lectures, attempts to present the outlines of his system to laymen, and divides his material into three parts: I. The Psychology of Error; II. The Dream; III. The General Theory of the Neuroses. The first part is, to our thinking, excessively and sometimes tediously elemental and prolix; but as the author advances into the subject his interest increases, and the latter part of the book will be found illuminating even to adepts. It is especially significant as being the first attempt the author has made to state his conclusions in a systematic and coherent form; for his voluminous previous publications have been scattered, so that it has been difficult to find some of them, and a few are out of print. Particularly interesting in this publication are the full and careful definitions of the points of difference between Freud's views and those of both Adler and Jung and the Zurich school. It is gratifying to note that the spirit of this discussion is well-tempered and philosophic, and without the bitter personalities that marked the inception of these divergences, which were so fortunate for the science of psychoanalysis but such a strain upon the early friendship of their respective leaders. It would be a calamity if Freud's repudiation of his more independent disciples should be as bitter as Wundt's was of his followers who

established the so-called Würzburg school, which he was as unable to follow as Freud is to accept the bold and often vague speculations of Jung. A recent writer has cleverly attempted to psychoanalyze these three leaders, making Freud the feminine; Adler, the masculine; and Jung the combining type,—and with much plausibility, so far as the first two are concerned.

*Psychopathology.* By EDWARD J. KEMPF. St. Louis, C. V. Mosby Co., 1920. 762 pp.

This indefatigable worker, the most prolific and original American thinker in this field, here brings together his own *aperçus*, hitherto scattered through various publications, into a more or less systematic whole, with very copious case-histories and eighty-seven illustrations. In its original form, Freudianism was developed from purely clinical data, with no more implications of a physiological background than introspection itself. But Kempf has attempted to supply this in what he calls the autonomic functions and gives us, with great cleverness, a correlation of these activities, including of course those of the endocrine glands, with the Freudian mechanisms. Higier in his "Vegetative Neurology," Eppinger and Hess in "Vagotonia" and others have been working in the same direction; but Kempf was bolder and went far beyond these men in applying the findings in this field to psychopathology. Everyone interested in the general field of psychoanalysis will find this volume indispensable.

*The Elements of Practical Psychoanalysis.* By PAUL BOUSFIELD. London, Paul, Trench, Trübner Co., 1920. 276 pp.

Of all the introductions to Freudianism which deal with elements, this is the latest and the best. The author dissents from Freud only with regard to his complete determinism as opposed to free will, his assumption that all dreams have the same causative factors, and thirdly his theory "that the sexual is the fundamental desire underlying all desires and emotions." He gives us a convenient glossary, but no index of his chapters, which are as follows: The Unconscious Mind, Desires and Psychic Energy, The Evolution of Erotic Desire, The Fate of Erotic Impulses and Aims, Parental Complexes, Narcissism, Dreams, The Fundamental Desires, Technique of Psychoanalysis (with an excellent account of the word-reaction method), Analysis of a Case of Compulsion Neurosis with Paranoid Symptoms, Criticisms of Psychoanalysis, Its Scope. Although this work is chiefly addressed to physicians who are laymen in psychoanalysis, it is of great interest to the general reader, and even the specialist will find much help in the judicious perspective and in the relationships which the author brings out.

*Mysticism, Freudianism, and Scientific Psychology.* By KNIGHT DUNLAP. St. Louis, C. V. Mosby Co., 1920. 173 pp.

The author groups Freudianism, spiritism, and Christian Science, "a siren trinity," as kindred forms of mysticism, and an "assault upon the life of the biological sciences which psychology alone is capable of warding off." In Chapter II, pp. 44 to 111, he gives a résumé of some of the Freudian positions, with quotations that show that he has made more effort than most critics of the system to understand it, which would serve as a good introduction for the beginner to a very limited portion of the field. In the long concluding chapter



he presents the "foundations of scientific psychology;" the chapter is largely expository of his views of awareness, apart from its objects, as the essential thing. We are told practically nothing about any of the Freudian mechanisms except repression and *Verschiebung*, and something of course of the Oedipus and Electra complex; he does not discuss infantile sexuality, sublimation, ambivalence, compensation, introversion and extroversion at all. Nor is there any delineation of the profound differences between Freud, Adler, and the Zurich school. There is little allusion to the later literature, and almost nothing of the applications of psychoanalysis to religion, literature, history, biography, etc. Evidently the author calls everything that deals with the unconscious, mysticism; just as the critics of Weismann called his constructions of metamicroscopic biological units, mysticism. What the latter, Christian Science, spiritism, etc. really are and mean, a subject which he rightly admits is a part of his programme, he unfortunately defers to a later publication.

In this book, but particularly in his also very well written "Personal Beauty and Racial Betterment," the careful reader cannot fail to see that the author has himself been not only greatly stimulated but also profoundly influenced by psychoanalysis. Beauty in woman he conceives as the possibility of motherhood; and in the second part he characterizes various anti-eugenic tendencies in the present, such as the withdrawal from the function of child-bearing of women who enter gainful occupations and those who go on the stage, etc. These views are effectively and very wholesomely presented; but the author does not seem to be aware of the fact that he is simply amplifying what is implicit in the whole psychoanalytic position, viz., that the chief function of the race is to transmit the sacred torch of life.

To our minds these booklets are distinctly the best of the author's always meritorious contributions to psychology, and he ought to recognize that Freudianism has been to him a very helpful mental stimulus. Moreover, in the last part of his *Mysticism* book he certainly points out diversities in what he calls scientific psychology which are as many and great as those in the system he criticizes; so that the thoughtful reader will close the book with the impression that "scientific psychology" is yet far from being scientific, because lacking a consensus even in so fundamental a thing as the definition and use of terms. Consciousness, which is his muse, has itself often been called first of the hetaerae; and those who worship at her shrine conceive everything unconscious not only as entirely outside the pale but as dangerous to scientific orthodoxy, as Bolshevism is thought to be to all forms of well-organized and effective government.

*The Adolescent Girl.* By PHYLLIS BLANCHARD. N. Y., Moffat, Yard and Co., 1920. 242 pp.

The psyche of the budding girl (*Backfisch*, *tendron*, "flapper") has seemed about the very most unknown of all the great domains of psychology. Woman has played a great rôle in culture history, from the days of the Pythonesses down to the Fox sisters, who gave the initial momentum to spiritualism in this country, and the Creary girls, who were the chief theme of investigation in the early years of the English Psychical Research Society. The author writes with a very wide knowledge of the literature of the subject, and has had much personal contact with girls in the pin-feather stage of their development. The book is therefore in some respects unique in its field, and is not only an excellent summary of what has already been done but

also makes important original suggestions at many points. The chapter headings are: The Broader View, The Sexual and Maternal Instincts of the Adolescent Girl, The Adolescent Conflict, The Sublimation of the Libido, Pathological Manifestations of Libido in Adolescent Girls, The Adolescent Girl and Love, and The Adolescent Girl and Her Future.

*Probleme der Mystik und ihrer Symbolik.* By H. SILBERER. Wien, H. Heller, 1914. 283 pp.

Here again, as in his great work on symbolism, the author takes his text from an old Rosicrucian manuscript on the parabola, and on the basis of its exegesis gives us a restatement of his views on the meaning of dreams and *Märchen*; and then attempts a specific psychoanalytic interpretation of his text, discussing more fully alchemy, the hermetic art, Rosicrucianism, free-masonry, introversion and rebirth, mysticism, and the royal art. This volume, although written in 1914, has only just reached this country, and so is included among the books that have appeared in 1920.

*Man's Unconscious Passion.* By WILFRID LAY. N. Y., Dodd, Mead and Co., 1920. 246 pp.

The author here gives us his third book, which is, like his preceding volumes, interesting and instructive; but while it contains much suggestive material, it presents no essentially new points of view. The chapters are as follows: The Total Sensation, Conscious and Unconscious Passions, Affection Is Not Passion, Insight, The Transfer of Passion, The Emotion Age.

*The Problem of the Nervous Child.* By ELIDA EVANS. N. Y., Dodd, Mead and Co., 1920. 299 pp.

The author is an experienced social worker who has for years come into close contact with childhood and has studied with Jung (who writes a very appreciative introduction to the book). She here gives us a picture of a Freudian child, with copious and very interesting illustrations. It is far and away more insightful, not only into the life of the child normal and abnormal, but even into psychoanalysis, at least from the Zurich point of view, than the child-psychology of Hug-Hellmuth which appeared a year or more ago. Despite the delicacy of some of the topics treated, there is little or nothing that could shock the most sensitive reader, so that we have in Mrs. Evans' pages a presentation of child-psychology which not only all parents but all psychoanalysts as well will profit by reading.

*Sex and Life.* By W. F. ROBIE. Boston, Richard G. Badger, 1920. 424 pp.

This book, by the author of "Rational Sex Ethics" and "Further Investigations in Rational Sex Ethics," is the largest and most important of his works. While not specifically Freudian, it would probably never have been written, and certainly never printed or read, but for the greater freedom of discussion and the fructifying new ideas that have come to us from Vienna and Zurich. The author begins with a very frank sex autobiography, advocates a correspondence school of sex education, and then proceeds to discuss rational sex ethics for parents, for young men, for young women, and for married people. There are copious and well-chosen references to suitable

literature for every class, and plenty of case-histories and glossaries. Dr. Robie in all his writings minimizes the evils of self-abuse. He paints very attractive pictures of the felicity of happy and fecund marriages. His own experience has been long, rich, and very varied, and he has learned how to draw lessons from it. His cases are not only interesting in themselves but are all the more so because drawn from typical New England communities with two characteristics that seem rather salient: first, the scrupulosity of the New England conscience; and secondly, infertility. The author's method of analyzing his cases is far simpler than that of the Freudians and seems generally to have been extremely effective. It may not perhaps be improper to add here that in his own personal family life he illustrates an exceptionally high type of living, and certainly seems to have found the way to a happy life and pointed it out to many others.

*Repressed Emotions.* By ISADOR CORLIAT. N. Y., Brentano's, 1920. 215 pp.

This is an interesting but rather light work which contains some interesting case-histories and some excellent generalizations. Corliat compares Freud's discovery of the unconscious to that of Harvey of the circulation of the blood, which made modern physiology possible. His chapters are: The Meaning of Repressed Emotion, Repressed Emotions in Primitive Society, Repressed Emotions in Literature, The Sublimation of Repressed Emotions, The Development of Psychoanalysis, The Depth of the Unconscious, A Fairy Tale from the Unconscious.

*Psychoanalysis: A Brief Account of the Freudian Theory.* By BARBARA LOW. N. Y., Harcourt, Brace, and Howe, 1920. 199 pp.

This epitome, prefaced and commended by Ernest Jones, is the briefest yet of the many introductions to Freudianism. The author treats, in successive chapters, the scope and significance of psychoanalysis; mental life, conscious and unconscious; reversions; the dream; social and educational results. The work is clearly and admirably written.

*Friedrich Hebbel: ein psychoanalytischer Versuch.* By ISADOR SADGER. Wien, Deuticke, 1920. 374 pp. (Schriften zur angewandten Seelenkunde, Heft 18.)

Sadger has added another to the nearly two-score psychoanalyses of great men in the above volume on Hebbel, the German poet and mystic. Psychoanalysis almost from the first has found in this writer's explorations into the depths of his own soul much material for quotation and comment; and Sadger's painstaking work here shows that in both his life and literature Hebbel furnishes some of the most striking illustrations of the Freudian mechanisms, and this in copious detail, so that few individuals yet analyzed come as near as he to the ideal case of *Gradiva*; while, on the other hand, the rather distinct stages through which he passed serve almost as well for this purpose as Maeder's study of Dante.

## NOTES

### THE ROAD TO EN-DOR<sup>1</sup>

This book tells the story of the plans laid by two British officers for escape from a prison camp in Asiatic Turkey. The first scheme, which took shape casually from some jesting trials of a home-made ouija board, was to convince the Turkish Commandant that the officers in question possessed mediumistic powers. It succeeded beyond expectation; not only the Turks, but also the fellow-prisoners of the pretended mediums were convinced. "In the face of the most persistent and elaborate efforts to detect fraud," writes the author, "it is possible to convert intelligent, scientific, and otherwise highly educated men to spiritualism, by means of the arts and methods employed by 'mediums' in general." More than that, these men may remain converted. "Eighteen months later, . . . I had told [one of the converts] all our work had been fraudulent, and had quoted [an instance] to show how it was done. . . . The Convert smiled pityingly at me"—and proposed an explanation by unconscious telepathy! No wonder, then, that the Turks were impressed. Things went so far that the camp was to all intents and purposes governed by the 'spook,' who secured many privileges for the inmates. And the plan finally failed, not by its inherent weakness, but through the over-credulity of its victims: a counter-spook, brought on the scene in the interests of escape, grew by an unhappy accident all too powerful; and it was the Commandant's fear of this opposing power that wrecked the whole elaborate device.

If, however, the first part of the book reads us an excellent lesson in applied psychology, the second part is even more instructive. The original plan, perforce abandoned, passed smoothly into another, a plan of release by way of pretended madness; the one officer became a 'furious,' the other a 'melancholic.' Here the risks were, of course, much greater than before, since the simulation must run the gauntlet not only of the local Turkish medical officers but also of the Paris-and-Vienna-and-Berlin-trained psychiatrists of Constantinople. The two men had been well coached at Yozgad by a fellow-prisoner, a physician of wide experience. They showed an astonishing endurance and persistence, and kept up an unremitting guard on their own conduct and on that of their visitors and attendants. The case-histories were also prepared with the utmost care and foresight. All the same, one wonders what might have happened if Mazhar Osman Bey had been less busy! Perhaps a psychiatrist may be persuaded to go over the data and tell us. This plan succeeded, and the officers were freed—just about a fortnight before the armistice with Turkey was signed. They had, nevertheless, the full satisfaction of success, and the record of their adventures is a valuable psychological document. E. B. T.

<sup>1</sup> *The Road to En-Dor: Being an Account of How Two Prisoners of War at Yozgad in Turkey Won their Way to Freedom.* By E. H. Jones, Lt. I. A. R. O. With illustrations by C. W. Hill, Lt. R. A. F. New York, John Lane. MCMXX. Pp. xiii., 375.



## JOURNAL DE PSYCHOLOGIE

The *Journal* of Pierre Janet and Georges Dumas, interrupted by the war, began to appear again in January, 1920. With the reissue, the editors have taken a new departure. Without any thought of hostility to British and American psychologists, they desire to make their magazine "a Latin journal, appearing at the same time in all the Latin capitals, publishing articles written by Latin psychologists, and addressing itself to all Latin readers." They have accordingly brought together an international staff, consisting of J. Ingenieros (Argentina), Van Biervliet and Decroly (Belgium), Austregesilo, M. Bomfim, J. Moreira, A. Peixoto, F. da Rocha (Brazil); G. Marañón, A. Pi Suñer, S. Ramon y Cajal, R. Turró (Spain); H. Bergson, Ch. Blondel, Chaslin, Delacroix, Lalande, Lapique, Piéron, Rabaud, Revault d'Allonnes, Séglas, H. Wallon (France); Boreas, Catsaras (Greece); Ferrari, Gemelli, Kiesow, Morselli, Ponzio, Rignano, Sante de Sanctis, Tanzi (Italy); Marinesco, Obregia, Radulesco-Motru (Rumania); and Bovet, Claparède, Larguier des Bancelles (Switzerland). Most of these names are already familiar to us; of the rest we shall hope to learn from the pages of the *Journal*.

E. B. T.

## ARCHIVIO ITALIANO DI PSICOLOGIA

We have received the first (double) number of this journal, which is edited by Professor F. Kiesow of Turin and Professor A. Gemelli of Milan with the co-operation of V. Benussi (Padua), L. Botti (Turin), C. Colucci (Naples), S. de Sanctis (Rome), E. Morselli (Genoa) and M. Ponzio (Turin). The staff is both strong and representative, and the *Archivio* has our best wishes for the success that it will undoubtedly achieve. According to a prefatory Note to the Reader, the *Rivista di psicologia*, edited by Professor G. C. Ferrari of Bologna, will devote itself to the application of scientific psychology and to the popularisation of psychological results among the students of neighboring disciplines, while the *Archivio* will publish strictly scientific articles. The contents of the present issue are: F. Kiesow, Observations on the relation between two objects viewed separately by the two eyes; A. Gemelli and A. Galli, Researches on attention: i. A new method for the study of fluctuations of attention; V. Roncagli, Experimental investigations by the method of the maze; G. A. Elrington, The expression of the musical intervals; L. Botti, Psychological observations on the concept of the 'last'; F. Kiesow, A phenomenon of central representation (assimilative illusion); F. Kiesow, A forgotten experiment (Fechner's rivalry between the dark field of a closed and the light field of an open eye); A. Gemelli, G. Tessier and A. Galli, The perception of the position of the body and of its derangements: a contribution to the psychology of the aviator. General Review: A. Gemelli, The application of psychological methods to the study of aesthetics. Notes.

E. B. T.

## SUBCUTANEOUS SENSATIONS

In the spring of 1920 I met with a minor accident that gave opportunity for observation of the sensations localized in the muscles when directly stimulated. The thumb and first finger of the left hand were cut almost completely through at the first phalange and the second or middle finger was completely severed through the second phalange. No pain was connected with the occurrence. As a matter of fact



several seconds elapsed before the injury was called to my attention by the loss of the usual sensitivity of the tips of the thumb and fingers. The lacerations were cleansed with sterile water before the experimentation, in order that the sensations might not be clouded by the effects common with the use of most germicidal preparations. The flow of blood was effectually stopped and the surfaces of the wounds were kept fairly free during the series of observations.

It was anticipated that any manipulation of the exposed surfaces would be painful to some extent; but pain was not observed, and adaptation was rapidly made to the new conditions. It was a rather excruciating feeling that was experienced, especially when large areas were stimulated by contact. The resultant sensations were at once recognized as possessing some of the qualities peculiar to the subcutaneous sensations, and it was attempted to take advantage of the circumstances to make an investigation of the nature of the sensations originating in the muscles.

The sensations resulting from the stimulation of the stump of the second digit and the proximal surfaces of the other two lacerations were very similar to the feeling ordinarily experienced when the skin is anaesthetised and pressure applied; but the resemblances were scarcely more noticeable than the differences. To compare the sensations directly does not convey an adequate conception of the nature of the sensations experienced when the muscle itself is stimulated. The sensations obtained in this manner were to some extent more intense, more noticeable, more distinct, and qualitatively somewhat different. The skin of the right forearm was anaesthetised and the two sensation-complexes were directly compared. Those arising in the bared muscles seemed to be more intimately related to the body, while the others were, comparatively, more aloof, more external, and seemed to be less a part of my 'self' than the former. The sensations originating in the anaesthetised area were qualitatively more complex; although, when a large area was stimulated on the bared muscles, the quality seemed to become increasingly complex with an increase in area or pressure or with a violent manipulation. The sensations arising from light stimulation of certain small areas on the exposed surfaces by the tip of a probe were of a nature that leads me to believe that in them I experienced isolated muscle-sensations simple and uncomplicated. There were only certain points at which these apparently elementary sensations could be aroused; but whenever these points were stimulated the unique quality was clearly perceived. Stimulation was found to be most efficient when the point was irritated by bringing a slight pressure to bear, at right angles to their axis, on neighboring fibres. The sensation can be described only by reference to the entire cutaneous and subcutaneous complexes. As a matter of fact its quality almost evades description.

The dull, deadened feeling characteristic of the sensations underlying the anaesthetised area does not carry over to the description of what appear to be the clean-cut sensations found by manipulation of the muscles themselves. The latter sensations, while not acute, are not heavy or torpid, but rather are keen and brisk, though accompanied by a 'smouldering' feeling. They are sharp and distinct, but not in the least painful or unpleasant, unless it be at first when they are novel and unexpected. They cannot be compared to the tired feeling following upon excessive muscular activity, as the muscle-sensations have been. On the contrary, a bracing feeling like that going with good physical tone serves better to convey some idea of

the nature of these sensations. They are bracing but bland. They are not diffuse, like the sensations arising from pressure on the anaesthetised area, but are fairly compact and unified.

A peculiar error of localization was observed. Only a very few of the stimulations were referred even to the approximate point of stimulation. They were usually referred to some point on the tip of the finger. In one instance the stimulation was localized at a definite point on the palmar surface of the finger tip, when in reality it was applied near the back. This one error was repeated several times, when the same point was stimulated without my being aware of the place of contact. The sensations leading to these faulty localizations were of a more complex nature than (and of a different quality from) what was considered to be the elementary sense-quality. These errors of reference still persist in the second digit, and it is now possible to hold the stump rigid and yet to have a definite feeling of flexion at the distal joint.

Similar observations of the nature of these deeper-lying sensations were carried out two months later, with the *abductor pollicis brevis* of the right hand uncovered, and essentially the same qualitative characteristics were noted as before, accompanied by even more marked errors of localization. In both regions the limen for temperature was very high, and at only a very few scattered points were temperatures that were not physiologically harmful perceptible.

These observations of difference in quality of the subcutaneous sensations when directly stimulated from their quality when investigated in the usual manner would seem to indicate that in the latter case we are still dealing with a complex from which only the surface sensations have been eliminated.

University of Iowa

DONALD A. LAIRD

#### THE INSTITUTE OF PSYCHOLOGY AT PARIS

At the University of Paris there has been inaugurated this year the Institute of Psychology under the Faculty of Sciences. The following courses are scheduled: Delacroix, *Psychologie générale*, "La Psychologie française au XIX siècle;" Dumas, *Psychologie pathologique et expérimentale*, "Introduction générale, theories et méthodes" and "Les suppléances sensorielles;" Janet, at the Collège de France, *Psychologie expérimentale et comparée*, "L'évolution de la personnalité;" Piéron, *Psychophysiologie générale*, "Études des sensations;" and Rabaud, "Introduction à la psychologie animale."

The Section of Pedagogy of the Institute offers a series of conferences at the Sorbonne by Rabaud, Meyerson, Piéron, Lalo, and Wallon; a laboratory course on neuro-psychiatric children at the Bicêtre; and a course on retarded children at the Asnières by Roubinovitch.

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## AN EXPERIMENTAL INVESTIGATION OF THE POSITIVE AFTER-IMAGE IN AUDITION<sup>1</sup>

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By HOMER GUY BISHOP

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Our problem is to find out if there is a positive auditory after-image, analogous to the positive after-image in vision, and in that case to describe it in attributive terms. If we detect the presence of auditory experience after the stimulus ceases, we must seek to identify this experience as after-image, or memory after-image, or memory image, or whatever it may be.

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<sup>1</sup> From the Psychological Laboratory of Cornell University.

*Historical*

The first experiments upon the after-effect of auditory stimulation appear to have been made by Mayer.<sup>2</sup> The sources of tone were tuning-forks supplemented by resonators. The tones were conducted to the one ear by means of a rubber tube; the other ear was plugged with wax. Between the nipple of the resonator and the free end of the conduction-tube stood a siren disk. When a space between the holes in the disk was before the nipple, the sound waves were blocked; when a hole was before the nipple, the sound passed into the tube at full intensity. As the disk was rotated, short periods of stimulation alternated with equal periods without stimulation. The critical value sought was the length of the interval between tones which would just be bridged, without loss of sensible intensity, by the after-effect, so that the *O* should hear a smooth tone.

Urbantschitsch<sup>3</sup> worked by a similar method. The tones were interrupted by a pendulum, which carried the one end of a conduction-tube to and fro past the limbs of a Y-tube. The tube to *O*'s ear, which completed the conduction system, was connected to the stem of the Y-tube. Unlike Mayer, Urbantschitsch sought to determine the critical interval at which the tones would just fall apart. The aim of the experiments was to measure the full duration of the after-effect, whereas Mayer measured only that part of it which showed no decrease of sensible intensity.

Schaefer<sup>4</sup> criticizes the method of interruptions on the ground that it cannot take separate account of the *Abklingen* and *Anklingen* of the tones employed. Marbe<sup>5</sup> also considers the method inadequate. "An investigation of the facts of *Abklingen* in auditory sensation can not be made . . . by means of successive, periodic stimuli, but only by the more difficult means of isolated auditory stimuli."

Urbantschitsch<sup>6</sup> further studied what he calls "primary" and "secondary" positive auditory after-images. The "primary" after-image is so closely joined to the sensation that there is no perceptible break between the two; the "secondary" after-image follows the sensation only after an interval, and may recur several times, at intervals of varying length. Urbantschitsch's method is so imperfectly reported that it is impossible to undertake a repetition of his experiments. We are told only that three tuning-forks were sounded at four intensities (characterized as "very strong," "strong," "moderately strong," and "weak") for periods of 5 and 15 sec.

## EXPERIMENTAL

We employed two sources of tone: Stern variators, with tube-transmission, and vibrating telephone receivers. It may be said at once that the principal difficulty with either source

<sup>2</sup> A. M. Mayer, "Researches in Acoustics," *Amer. Jour. of Science and Arts*, 147, 1894, 1 ff.

<sup>3</sup> V. Urbantschitsch, "Ueber das An- und Abklingen acustischer Empfindungen," *Archiv. für d. ges. Physiol.*, 25, 1881, 323 ff.

<sup>4</sup> K. L. Schaefer, Nagel's *Handb. der Physiol.*, 3, 1905, 507.

<sup>5</sup> K. Marbe, "Akustische Prüfung der Thatsachen des Talbotschen Gesetzes," *Archiv für d. ges. Physiol.*, 100, 1903, 557.

<sup>6</sup> V. Urbantschitsch, *op. cit.*; and "Zur Lehre von der Schall-empfindung," *Archiv für d. ges. Physiok*, 24, 1881, 585 ff.



was to secure a clean cut-off of the stimulus-tone. Our successive arrangements, therefore, represent repeated efforts at improvement in this respect. The experiments were performed in a suite of three rooms. It was necessary to spread the apparatus over a wide space on account of the power of tones to penetrate even heavy stone walls. Accordingly the *O's* sat in one room, *E* operated the apparatus in another, and a large unoccupied room lay between. Even so we found that the sources of the higher pitches at greatest intensity must be enclosed in soundproof boxes.

#### *Series I*

In our first series of experiments the source of tone was the variator. Four pitches were used, 1024, 512, 256, and 128 vs. The variators were blown at three intensities, and for three durations of stimulation. The three intensities were obtained by setting the air-pressure at 15.0, 7.6, and 4.0 mm. of water for the pitch of 1024 vs.; at 5.0, 2.6, and 1.6 mm. for 512; at 19.0, 11.6, and 8.0 mm. for 256; and at 8.0, 5.5, and 4.0 mm. for 128. The nozzle of the variator was so adjusted to the mouth of the cylinder that a maximum of intensity should require the lowest possible pressure. We found that the pressures could not be equated, but varied from instrument to instrument, as indicated by the pressure-values just listed. The durations were 5, 15, and 30 sec. read directly from a Pye clock. Since the seconds-hand of this instrument moves over a dial of 11 cm. diameter, the spaces between seconds are broad enough to admit of accurate reading.

A special control enabled us to adjust the air-pressure quickly and accurately. A wooden lever 85 cm. long, screwed to the handle of the rotary valve, swung before a ruled pressure-scale. From one side of the lever and 60 cm. from the valve, two metal arms (2 cm. long and 0.5 cm. broad) extended parallel to each other and at right angles to the edge of the lever. They were 4 cm. apart, and a fine wire was drawn taut from the outer end of the one to the outer end of the other. This wire, parallel to the edge of the lever and to the lines of the scale, lay on the surface of the scale in such wise that the error of parallax was avoided.

The sound-waves were picked up by a funnel of cardboard and reflected into a brass conduction-tube of 1.9 cm. inside diam. and 0.15 cm. thickness. The small end of the funnel fitted snugly over the end of the brass tube; the diameter of the large end was 15.0 cm.; and the length of an element in its surface was 45.0 cm. This slant is that of an old model of Victrola horn, and is approximately the same as the slope of the funnel in Marbe's *Sprachmelodie-Apparat*. When the variator was set in position, its mouth was in line with the axis of the funnel, just not touching the edge.

The brass conduction-tube was 8.76 m. long. It passed through two walls, across the middle room of the suite, and ended in *O's* room in an iron pipe 2.3 m. long. The iron pipe, which lay horizontally against the wall and at right angles to the brass tube, served as distributor of the tones to the four booths in which the *O's* sat. Four nipples were tapped into it, and from these four rubber tubes, of 0.6 cm. inside diam., led to four pairs of stethoscopic ear-pieces. The brass tube



entered the iron pipe at its middle, and the nipples were at 36 cm. and 104 cm. right and left. Under these conditions, the intensity at all listening tubes was sensibly the same. The booths in which the *O*'s sat were separated by heavy curtains.

We have described above the valve which controlled the blowing pressure. It was necessary to introduce another valve between the pressure-valve and variator, which should cut off the air suddenly and bring the tone to a sharp ending. The special valve constructed for this purpose somewhat resembled the piston-valve of a cornet. In one position of the piston, the air went straight through the one opening to the variator, while the other opening was closed at the tube in which the piston moved. In the other position, the passage was reversed; the hole which had been open was now closed, and conversely. In this second position, no more air could enter the part of the tube between valve and variator, and the air which was present under full blowing pressure when the valve first closed had two outlets, the one by way of the nozzle of the variator, the other by way of the passage through the valve. Since the latter passage offered the lower resistance, most of the pressure was spent in this direction, and the terminal "whoop" which otherwise was very troublesome to the *O*'s was reduced to the vanishing point. (Unless the air escaping from the tube, in the process of reduction of the pressure within it to atmospheric pressure, can be diverted from its course through the nozzle, the pitch of the tone in dying falls quite perceptibly as the pressure diminishes.) The diam. of the holes through the piston, 0.6 cm., was the same as the inside diam. of the rubber tube carrying the air to the valve; so that the piston need be moved no more than 0.7 cm. to open the one passage and close the other. In the first series of experiments we threw the valve by hand.

As warning signals we used four small 4-volt incandescent lamps placed upon the wall before the *O*'s. The circuits were so arranged that the light could be made to glow at two intensities. The lamps were turned on at the lower intensity at the beginning of the experimental hour, and remained at this intensity throughout, except when they were flashed as signals; at the lower intensity they showed merely a dull red glow. Our object was, by keeping the filament constantly warmed, to increase the sensitivity of the lamp. We needed to have it flash brightly in an instant, an impossible result if the filament were cold at the outset. *E* closed a key to flash the usual "Ready," "Now" signals. When the air was turned on at the valve, the same movement closed a mercury-contact key attached to the piston of the piston-valve, and the lights burned brightly. When the air was cut off, the movement of the piston broke the circuit, and the lights dropped to the lower intensity. This method secured temporal coincidence of the cutting-off of the tone and the dimming of the lamp.

Every *O* had at his hand a key making or breaking the circuit in the recording apparatus. This apparatus consisted of eosin writers adapted to use with ticker paper. They were made of tin; and though different in structure their principle of operation was that of the draughtsman's ruling pen. They wrote from the horizontal position, withstood hard knocks, and did not spill the fluid when tapped rapidly against the paper. When *O*'s key was closed, the tip of the writer was pulled against the paper by an electromagnet, and a line was written representing the length of any after-effect which might be experienced. When *O* was ready for the next tone he tapped the key as a signal to *E*. The writers were so slender that we were able to

write one time-line and four record-lines upon the usual ticker paper 1.8 cm. in width.

The dashes in the time-line were made by a Kronecker interrupter vibrating in tenths of sec. To fix the exact moment at which the tone began, we resorted to another mercury-contact key upon the piston of the piston-valve. When the air was cut off, the circuit was closed, and the time-writer wrote a continuous line; when the piston moved to turn on the air, the circuit was broken, and under power from another circuit the interrupter-point began to write tenths of a sec. The paper was drawn by a motor which ran throughout the experimental hour; but the feed of the paper was controlled by a clutch, and it was drawn past the writer only when needed. With this arrangement there was no lag of the paper; it was drawn immediately at full speed.

*Observers.* The O's were Mr. R. T. Holland, graduate student and assistant in the department; Miss M. F. Martin, Dr. C. W. Perky, Miss A. H. Sullivan, and Mr. S. Takaki, graduate students majoring in Psychology. Observer P was somewhat more highly practised than the others, but all were experienced O's.

*Instructions.* "You will hear a tone which will begin when the signal light brightens, and will cease abruptly as the light dims. You are to note whether the tonal sensation continues or recurs after the dimming of the light. If it continues, you are to press the key immediately and release it at its final disappearance. If it recurs, you are to press the key at every appearance and hold it until the disappearance of the recurring sensation. When the experiment is over, you will write a descriptive account of the after-sensation or after-sensations on the blanks provided."

For every pitch there were 9 stimulations presented in the order: strong-short; middle intensity-middle duration; weak-long; strong-middle duration; strong-long; middle intensity-short; middle intensity-long; weak-short; weak-middle duration. This series of 9 stimulations was run off first with the highest pitch, then with the remaining three pitches in descending order. There were thus 36 experiments in a series. Every series was repeated five times, giving a grand total of 180 experiments for every O with this first arrangement of apparatus.

### *Results*

Our results showed a considerable number of cases in which the tones "ended abruptly," were "chopped off," with no trace of terminal modification or after-effect. Besides these, however, there were cases described in which the ending of the tone was modified, in quality, in intensity, or in temporal

course. The *O*'s spoke of "slow dying," "prolonged ending," "not abrupt," a "whoop." Never did the modified ending carry sensibly beyond the dimming of the light, or suggest to *O* the continuation of the sensation in a positive after-image.

Table I is a summary of all modified endings. Unfortunately, we cannot be sure of their interpretation. The slow cooling of the filament in the lamp may have led to error in judgment on the part of the *O*'s. The apparatus could not guarantee that what was heard was the natural ending of a suddenly stopped tone; so that these modified endings may

TABLE I

O	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	22	18	7	17	14	16	18	14	14	1	47
M	30	27	29	29	32	25	28	20	20	18	86
P	36	29	22	27	30	30	24	23	27	13	87
S	37	32	38	39	38	30	35	34	16	20	107
T	30	25	20	26	26	23	22	19	23	11	75
Total	155	131	116	138	140	124	129	110	100	63	402

in fact have followed the last vibration, and thus may represent an after-effect. Besides this serious defect in the apparatus, our first series of experiments indicated that several minor changes were necessary. Noises in *E*'s room reached the *O*'s through funnel and conduction tube; the purr of the motor which drew the ticker paper had in it a note that was easily mistaken for an after-image; the hand-operated valve was not always thrown with equal speed from the one position to the other; and any echo of the tones in *E*'s room might reach the *O*'s.

#### *Experimental: Series II*

We chose the lights as signals because they were silent; but they were not reliable; and we now substituted for them small electromagnets from a player-piano. These magnets are not noisy; and when operated by a weak current, just strong enough to produce a light, quick movement, they did not annoy the *O*'s. To the armature of the magnet we soldered a strip of tin, 4 cm. by 0.5 cm., to serve as signal flag. Every *O* had a magnet and flag in his booth before him, at the place where the light had been. When the current was off, the flag stood almost vertically; when it was turned on, the flag was pulled quickly down to the horizontal.

In order to operate the piston-valve quickly and at a constant speed at all times, we built two solenoids end to end on a single base. They were mounted at the end of the piston, and an extension of the piston passed through them both. One solenoid pulled the piston to turn the air on, the other pulled it back again to turn the air off. With a sufficient current the shift was almost instantaneous.

We shut out the noises from *E*'s room by placing the variators in a sound-proof box. We cut the funnel down to a smaller size (diam. 10 cm., length 25 cm.) for use inside the box. The whole conduction system was now isolated, though there was still the possibility of echo from the walls of the box. Indeed, we found that there was a faint tone which made its appearance regularly at a short interval after the air was cut off. We satisfied ourselves that this tone was in the apparatus, and was not subjective, by making records of the vibrations. A diaphragm of the Marbe *Sprachmelodie-Apparat* was held firmly against *O*'s end of the conduction tube, and the smoke-rings registered the tone.

To rid ourselves of this source of error, we so hinged one of the sections of brass conduction-tube that the one end of it could be drawn aside. The other end was attached by a heavy rubber tube to the fixed end of the next section of conduction-tube. An electromagnet in series with the solenoid which cut off the air to the variator pulled this swinging section of tubing to one side at the same time that the variator ceased to be blown. In this way the echo was diverted into the middle room of our suite, and the *O*'s could not hear it. We found it very difficult to swing the pipe noiselessly, but we finally hit upon the following arrangement. One end of a thin strip of wood, about 1 m. long, was fastened to the floor with a hinge; the other end carried the swinging end of the conduction-tube. The electromagnet was fastened to a firm support about 30 cm. from the floor. A piece of iron, screwed to the thin strip at this level, was attracted by the magnet, and the pipe was thus drawn aside. The iron was faced with felt; and by this means, supplemented by the elasticity of the strip of wood, the pipe was brought to rest silently in its new position. The supporting wooden strip stood so nearly vertical that it was easily pulled toward the magnet; but it was inclined so far away from the vertical that gravity pulled it back to the first position when the circuit to the magnet was broken. A piece of spring steel, faced with felt where it touched the wooden strip, furnished a silent stop for the pipe when it returned to the position for conduction. The swinging section was 1.82 m. long; with a radius so large, and an outside diam. of the tube of only 2.2 cm., the space between the swinging end and the fixed end of the next section could be about 0.25 mm. The space was adjusted at every experimental hour so that the ends should just not touch. The break in the pipe was 84 cm. from the point at which the brass tube joined the iron pipe in *O*'s room. We felt satisfied that, with a distance so short as this, no residual sound-waves remaining in the conduction system between the *O*'s and the break in the tube could have any effect for audition.

In the light of our first results, it is clear that the warning signal must be very accurately timed. The modification in the ending of the tone was so short that the flag must move in exact coincidence with the last sound-wave. We turned again to the Marbe apparatus. It was set up with two diaphragms and one gas-burner, all in the same gas system. One diaphragm made the tone-rings, the other the ring from the flag. The diaphragm at the flag was brought so near the flag that the first beginning of movement was recorded by the membrane. By this arrangement the effects from the two membranes were superimposed. The record should show a band of regular rings representing the tone, and then an aperiodic ring written by the flag, followed by a band with no rings. By carefully adjusting the contact placed where the swinging section of conduction-tube would close the

circuit to the flags, we were able to secure this kind of record. The heavy ring from the flag showed as the last ring in the series, and it stood at the proper distance from the last ring but one to be in coincidence with the last tone-ring. To be sure, the record did not indicate whether the heavy ring was the result of a summation of two rings; but it did show that the flag was not more than one vibration out of step with the tone (256 vs.). Still, however, there was the possibility that the stroke of the flag blotted out the tone-rings; tone might be present, but unrecorded. We put this possibility to the test by allowing the flag to make its ring without cutting off the tone. We found that the tone waves were just as distinct in the record after the stroke of the flag as they were before. The disturbance in the flame set up by the flag did not blot out the effect of the tone, but gave a superposed effect.

With these improvements in the apparatus we were ready for the next set of experiments.

*Observers.* The O's were H, M, S, T of the previous group.

*Instructions.* "You will hear a tone which will begin when the signal flag falls, and will continue for a varying period of time. About a second before the tone ends, the flag will fall as a warning signal that the tone is about to end. Just at the instant that the tone ceases abruptly, the flag will fall again. You are to note whether the tonal sensation continues or recurs after the flag falls. If it continues, you are to press the key immediately and release it at the final disappearance of the after-sensation. If it reappears, you are to press the key at every appearance and hold it until the disappearance of the recurring sensation. When the experiment is over, write a descriptive account of the after-sensation or after-sensations on the blanks provided."

The experiments in this series were conducted with the same tones as before, in the same order. Except for the changes in the apparatus, already described, and for the warning signal given during the course of the tone, the method is the same as in the first set of experiments.

### Results

The distribution of the modified endings is shown in Table II. The decrease in number between Series I and Series II

TABLE II

O	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	5	1	0	2	3	1	2	0	4	0	6
S	35	28	26	36	32	21	32	30	18	9	89
T	6	2	2	7	3	0	4	2	0	4	10
M		(none)			(none)			(none)			
Total	46	31	28	45	38	22	38	32	22	13	105



seemed to indicate that further refinements might lead to their complete elimination. We, therefore made additional efforts to refine our technique.

*Experimental: Series III*

So long as the tones were stopped by cutting off the air, there was escape of the air under pressure between the nozzle of the variator and the piston-valve. An unknown part of this air must certainly escape by way of the nozzle; and its escape, if audible, might lead to O's reports of modified endings.

Only after we had discovered how difficult it is to avoid the influence of the diminishing air pressure, did we turn to the rather obvious plan of allowing the variator to blow continuously, while we broke the path of conduction in order to obtain the periods of silence between stimulations. We accomplished this by swinging more sections of the conduction-tube. The section at the sound-proof box was pivoted at the middle to allow the ends to move up and down. The fulcrum was a wad of cotton, packed rather tightly around the pipe in the hole in the stone wall through which it passed. The next section was joined to this lever of conduction-tube and to the third section in the line by rubber tubing fitted over the ends of the pipe. When the end of the pipe in E's room was thrown down, the end in the middle room went up, carrying with it the end of the second section. This movement produced a break in the line at the box, but preserved the continuity in the next room. At the other side of the room was the section adjusted to swing in the previous set of experiments. The swinging end was now supported by a wire and pulley, so that it could be drawn up and dropped down. The means of obtaining simultaneous and equal movement at both breaks in the conduction system was very simple. The wire just mentioned extended across the middle room; its ends dropped vertically down from the pulleys and were fastened to the conduction-tube, the one end to the end of the lever section, the other to the free end of the swinging section. When the end of the lever at the variators went down, the end in the next room went up by an equal amount, since the fulcrum was in the middle. This upward movement allowed the wire to roll over the pulleys, and the end across the room dropped by an amount equal to the movement of either end of the lever section. The displacement thus secured was about 5 cm. The pull upon the pipe was exerted by means of a solenoid fastened to the floor in E's room. A cord tied to the end of the lever-section supported the core at a good pulling distance above the center of the solenoid. With a strong current, the displacement was sudden and free from noise. We found it necessary, however, to sew the core of the solenoid into a close fitting bag, in order to secure silent action of the core within the coil.

It is obvious that the shape of the ends of the conduction-tube at the break could not be circular. The diminishing amount of area for conduction as the ends became separated might be responsible for an observable decrease in intensity of the tone. The ends of the tube, except for a slit of 1.8 cm. by 0.3 cm., were accordingly plugged with sealing wax. The long dimension of the slits was horizontal, so that a quick movement of the end of the pipe downward brought them wide apart, too rapidly for any sensible decrease in intensity. We tried at first to use four of these slits, but found that they reduced

the intensity; two at the sound-proof box were sufficient. The circuit driving the signal flag was again closed by the swinging pipe.

We found that the two high tones could be heard three rooms away, even though the variators were in a box with very heavy double walls, if they were sounded at more than our intermediate intensity. We accordingly decreased the blowing pressure to 2, 4, 7.6 cm. of water for the tone of 1024 vs.; and to 0.9, 1.6, and 2.6 cm. of water for that of 512 vs. The two breaks in the conduction-tube weakened the low variator tones beyond usable limits. In their stead we used tuning-forks (with resonators) of 256 and 128 vs. The resistance in the circuit was such that at 10 v. we obtained satisfactory driving power for the forks at 1, 2, and 3 amps. for 256 vs.; and at 2, 3, and 5 amps. for 128 vs.

### Results

The results from this series of experiments are shown in Table III. We reserve comment upon them until later.

TABLE III

O	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
S	30	26	20	25	27	24	30	33	7	6	76
T	1	3	4	3	3	2	3	3	1	1	8
Total	31	29	24	28	30	26	33	36	8	7	84

### Experimental: Series IV

The apparatus just described was satisfactory except for the fact that the tones were weak. As it stood we could be reasonably certain that the *O*'s heard the natural ending of a tone, the sensible process of transition from tone to silence. The noise incident upon swinging the pipes was, it is true, barely noticeable just after the ending of the tone was heard, and a very faint after-image might have been lost. But the main fault lay with the intensity of the stimuli themselves. We required a source which should have fairly intense tones, and which should be completely silent in the period following the cessation. After a good deal of cut-and-try experimenting, we had recourse to telephone receivers.

The instrument was the ordinary variety of receiver of 75 ohms resistance. It was actuated by a Petzold double induction coil, with a current of 2 amp. at 6 v. through the two primaries connected in series. The vibration frequency in the receivers was determined by the number of interruptions in the primary, and not by a separate source of sound. By this method we avoided the complications which a transmitter would have introduced. The interrupters were tuning-forks of 435, 350, 256, and 100 vs. We were unable to drive the forks satisfactorily by the same current which passed through the primaries of the coil; by means of an extra pair of contacts put upon the forks, we passed two separate currents through them, the one driving the fork, the other leading to the induction coil. A condenser connected across the point of interruption in the circuit to the coil kept down the sparks at the interrupter, and gave a tone free from the usual click and buzz of receivers.

The line across which the receivers were connected derived its power from the secondary coils connected in series. A special switch with tion. After a good deal of cut-and-try experimenting, we had re-breaking both sides of the line at once. When one side alone was broken, the receivers sounded faintly; and they could be made silent only by interrupting both sides of the line together. This same switch also carried contacts controlling the signal flags. When the path to the receivers was broken, the circuit to the flags was made, just far enough in advance to counteract the slight lag in the flags.

The three intensities of stimulation were obtained by setting the secondary coils both at 0; the one at 14, the other at 2; and both at 14. There was no further change in the experimental procedure.

### Results

Table IV contains a summary of modified endings from Series IV of 360 observations for every O.

TABLE IV

O	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	120	120	0	80	80	80	60	60	60	60	240
S	119	120	113	120	118	114	90	89	87	86	352
T	6	2	1	2	3	4	1	0	4	4	9
Total	245	242	114	202	201	198	151	149	151	150	601

### Significance of Results

So far as concerns a positive after-image, analogous to the positive after-image in vision, we have nothing to report. In a total of 3,780 observations, the results have been flatly negative. But our 'modified' endings are positive results; and as they have been ineradicable, we are obliged to discuss them.

The instructions laid emphasis upon the positive after-effects of our stimuli; and the different O's took different attitudes toward the endings of the tones. In Series I and IV, H reported the modified endings frequently; in Series II, he was especially attentive to after-effects, and only the more pronounced of the modified endings were reported; in Series III, he made the general statement that he could hear a "ylup," if he made an effort to hear it, coincident with the drop of the flag. Throughout the series he took the instructions literally, and reported "Nothing" for the after-effect of every stimulus. M never reported modified endings after Series I, and like H never heard an after-effect. T reported modified endings very infrequently after Series I, but he never totally ignored them. S failed always to hear after-effects, but she persistently gave her attention to modified endings.

An examination of the totals in Tables I to IV shows that for every series the frequency of the report of modified endings is directly proportional to the degree of intensity of stimulus. Even in Series III, where the strongest stimulus was rather weak, the rule holds in some measure. The influence of duration is less regular; but there are always fewer modified endings reported at 5 sec. than at 15 and 30 sec. In the case of pitch, the tables may be misleading. In Series I and II, decrease in frequency of modified endings parallels a fall in pitch; but the high tones were intense and the low were weak; so that what might be taken for an effect of pitch is in fact an effect of intensity. We feel assured, from the results in Series III and IV, that this interpretation is correct. In both these series all pitches were of nearly equal intensity, and the distribution of modified endings is also nearly equal for all pitches. The contradiction in Table III is only apparent. It was very difficult to keep the tuning-forks of 256 and 128 vs. vibrating at low voltages, and for this reason we gave only one series with each. At higher voltages the forks had so great an amplitude of vibration that they struck the core of the electromagnet; for this reason it was impossible to grade the intensities from a higher maximum.

Throughout the earlier series, we regarded the modified ending as due to a fault in technique. We believed that refinement of apparatus would remove it. The agreement between number of modified endings and intensity of stimulus supported this opinion; but the influence of duration was against it. In order to explain the effect of duration, we searched the results to find what terms were used in describing the modified ending. Many references to it were not descriptions, but characterizations like "swoop," "whoop," "sigh," "groan," etc. In Series III, H and S agreed that the difference between the tone and its ending was, in part, one of 'vocality,' which changed as the tone went off. S further described the ending as having "less body than the tone;" it "disappeared out in space," "decreased in volume," "thinned out," "lost quality," etc. We have set down the attributive conditions of stimulus under which such descriptive terms were used in Table V. The results are all taken from Series III, since it is in this series only that the O's maintained a constantly descriptive attitude.

The uniform distribution for duration shows that the modified ending is not dependent upon differences in that attribute of stimulus.

TABLE V

	INTENSITY		
	Strong	Medium	Weak
O			
H	120	120	0
S	37	41	43

DURATION			
O	30	15	5
H	80	80	80
S	40	41	40

PITCH				
O	435	350	256	100
H	60	60	60	60
S	33	42	26	20

In the same group of experiments, certain terms indicative of judgments of intensity, like "pronounced swoop" and "less üh," were used. Their distribution is shown in Table VI.

TABLE VI

	INTENSITY		
	Strong	Medium	Weak
O			
H	120	120	0
S	10	5	3

DURATION			
O	30	15	5
H	80	80	80
S	6	6	6

PITCH				
O	435	350	256	100
H	60	60	60	60
S	10	2	1	5

The extreme regularity of H's results is due to a 'set' for intensity. He reported only upon the relative intensity of the modified endings, which he found to be well above the limen. The figures show again that the modified endings are independent of duration. In Series I, where S frequently gave judgments of length of modified ending, the same result was found: the modified ending was short for all durations of stimulus.



It will be remembered that in every series every degree of intensity was given once with every duration of stimulus. In Series III, we counted the number of times that S used the same term to describe the modified ending for all three durations in a series or for all three intensities. The number of cases is shown in Table VII.

TABLE VII

INTENSITY			
Strong 12	Medium 8	Weak 1	Total 21
DURATION			
30 1	15 2	5 2	Total 5
PITCH			
435 9	350 6	256 2	100 9

It is evident that duration does not markedly affect the modified ending, seeing that for 12 of the 15 times that the strong stimulus was given *O* is able to call the effect by the same name in spite of the 3 different durations of stimulus. The single case recorded in column 3 is very misleading in that 7 of the 12 weak stimuli were negative, *i.e.*, ended abruptly for sensation, leaving only 5 times that a single term could be used. The table shows that duration had a slight influence in determining the descriptive term; but it is only about one quarter as effective as intensity. The 7 negative cases were distributed, 5 to the short, and 2 to the intermediate duration.

In Tables I to IV, difference of duration appears to be effective in determining the number of reports of modified ending, and may, therefore, be a factor in determining their existence or non-existence. In Tables V to VII, duration does not modify the character of modified endings. Our experiments offered the suggestion that pressure in the ears<sup>7</sup> may have been of importance in forcing attention to the modified ending. T reported pressure or deafness following the stimulus under conditions which are shown in Table VIII.

<sup>7</sup> On the perception of silence, see E. B. Titchener, "A Further Word on Black," *Jour. of Philos. Psychol. and Scient. Methods*, 13, 1916, 649 ff.

These results are all taken from Series IV, in which the stimuli were sufficiently strong to make pressure and deafness moderately conspicuous. We conclude from them that both intensity and duration co-operate to bring about a state called sometimes deafness, sometimes pressure. The only time that a weak stimulus produced pressure was when its duration was 30 sec. Here, then, is an effect of duration which may be responsible for its otherwise unexplained influence upon the frequency of report of modified endings. Through it, only those modified endings will be reported which force themselves to the focus of attention. Our instructions, however, tended to direct attention to the period following the ending of the tone. Hence we should expect a report of pronounced or conspicuous endings, but of no others.

TABLE VIII

INTENSITY				
Strong	Medium	Weak		
19	18	1		Pressure
4	7	0		Deafness
23	25	1		Total
DURATION				
30	15	5		
18	12	8		Pressure
6	2	3		Deafness
24	14	11		Total
PITCH				
435	350	256	100	
19	4	9	6	Pressure
4	3	4	0	Deafness
23	7	13	6	Total

In order to substantiate this explanation, it becomes necessary to show, by a special direction of attention, that modified endings are, in reality, always present. H had already said that he could hear them with every stimulus, if he tried. Prof. H. P. Weld (W), and Dr. L. B. Hoisington (Ho.), members of the department and highly experienced O's, had previously been called in for supplementary observations following Series III. At first, they reported modified endings, but when they assumed a passive attitude toward them, and directed their attention to the silence following the end of the tone, the number of modified endings observed became fewer. Their reports indicated, nevertheless, a possibility that all endings of tone are modified.

To put the matter to the test, we made a few experiments, with Dr. K. M. Dallenbach, instructor in the department, H, Ho, M, T, and B (the writer) as *O*'s.

*Experimental: Series V*

The stem of the Y-tube at the ear-pieces was held in the left hand; and the end of the rubber tube, which previously had been joined to the Y-tube, was held in the right. Both free ends were brought together, just not touching, for stimulation, and were suddenly drawn apart to remove the stimulus tone. The movement was made in all possible directions, with equal effect upon the auditory experience; and the thumb held against the end of the rubber tube to cut off the tone had no different result from that noted with movement of the tube. These movements provided a soundless means of discontinuing stimulation. *O* was asked, first, to direct his attention to the change in the character of the tone just as it ended, and to report whether it could always be heard; secondly, to turn his attention to the period of silence following the tone, and to report, from stimulation to stimulation, whether the silence seemed more or less distinct from time to time; and thirdly, to report the times when the modified ending especially attracted attention. Stimulations were given for 5, 15, and 30 sec.

*Results*

Every *O* was able to hear a modified ending of every stimulation, if attention were directed upon it, even with weak tones carried almost to the limen. Every *O* found that the stimulus of 5 sec. was incapable of producing any but the faintest pressure which is characteristic of perceiving silence. Durations of 15 and 30 sec. definitely produced the pressure; but the *O*'s did not agree whether 30 or 15 sec. produced the greater pressure. Every *O* found also that the modified ending was not insistent after the 5 sec. stimulation, but that it stood out very conspicuously from stimulations of 15 and 30 sec. In B's experience the heaviness of the deeper silence from long stimulation worked, as if by contrast, to make the modified endings stand out more vividly without necessarily becoming more intense. It seemed to be the 'contrast' between silence and modified ending which grew more intense as the stimulus-time was lengthened. Since the pressure is cumulative with the product of intensity and duration of stimulus; and since the intensity of the modified ending seems to depend upon intensity of stimulus; we conclude that the enhanced 'contrast' effect comes by way of increased pressure.

This, then, seems to be the explanation of the apparently contradictory results. The modified ending varies in intensity directly with intensity of stimulus, and is always present. When the *O*'s report abrupt endings, or fail to report the end-

ing at all, their direction of attention to the period of silence is complete; and no endings, except the most vivid, come to their notice. Long stimulations enhance the contrast-effect between tone and silence, and increase the number of times that *O* hears the modified ending; they thus give the impression that the longer the stimulus-time, the more frequent is the modified ending.

#### *Nature of the Modified Ending*

It is tempting to regard the modified ending as 'subjective' in nature, and to identify it with the *Abklingen*<sup>8</sup> or 'drop' of auditory sensation which Mayer and others have attempted to measure. This view is supported by the facts that the modified ending varies directly with intensity of stimulus, and that it shows always a thinning-out or reduction of the 'body' of the tonal complex. While, however, we have no wish to dispute the presence of an *Abklingen*, we are nevertheless of the opinion that, in our experiments, this subjective factor was outweighed by objective changes.

In the first place, we mistrust the results from the telephones. So long as the tone is stopped by taking away the driving power of the diaphragm, there is a source of error remaining: the diaphragm must come to rest from the position in which it happens to be when the current is cut off, and this return is definitely audible. Our arrangement with induction coil and condenser served to eliminate the noisy click natural to the instrument at the moment when it ceases to be under the influence of the electromagnet; but it is evident that the very fact of vibration still necessitates a return to a state of equilibrium.

For a similar reason, the results from the variators are not free from objection. When the variator ceases to blow, we have no means of knowing through what stages the physical stimulus passes in coming to rest. At one moment the air is in motion, and a moment later it comes to rest. Our modified endings may depend in part upon objective change in the sound waves. When the path of conduction is broken by pulling the conduction-tube aside, eddy-currents must be set up around the ends of the tubes; and these, though not audible at low intensities, may change the character of the stronger tones as they end.

<sup>8</sup> For *Abklingen* in general see C. Stumpf, *Tonpsychologie*, 1883-90, references in index: K. L. Schaefer, Nagel's *Handb. der Physiol.*, iii., 1905, 504 ff.

There are, indeed, positive indications that the modified ending is in part objective. (1) The results from Series I-III differ widely with respect to the number of times the pitch changed in any direction. A summary is given in Table IX.

TABLE IX

	SERIES		
	I	II	III
Pitch rises.....	6	25	48
" the same.....	48	15	23
" falls.....	11	7	5
Total .....	65	47	76

In Series I, the vibration in the air spent itself normally. In Series II, when the pipe was drawn aside, the path of conduction was interrupted; and eddy-currents at the break may have changed the character of the ending of the tone. The ends of the tube were not plugged; and even though the pipe moved quickly, a certain brief time was required for the movement. In Series III, the ends were plugged, and the movement of the pipe was quicker than in Series II. The eddy-currents would be more intense, owing to the greater speed with which the pipe moved and to the narrowed opening in the pipe. Table IX shows that the increase in the number of judgments of higher pitch, and the decrease in the number of lower pitch, are correlated with the suddenness of the ending of the tone and the violence of the movement of the pipe. (2) We made a few experiments as a check upon the implication of Table IX by the method of pulling the rubber tube away from the stem of the Y-tube at the ear-pieces. When the end of the rubber tube vibrates rapidly before the end of the Y-tube, the movement produces a distinct thud or noisy puff. If the movement be made sufficiently violent, the noise covers up the tone. B, H, Ho, M, and T all reported that the modified ending was most intense, at any intensity of stimulus, when the rubber tube vibrated 4 to 6 times per sec. This result proves that long stimulation does not add to the intensity of the modified ending, but that rate of vibration does, probably from the increase in energy of the stimulus due to interference of air-waves at the ends of the tubes. (3) B and Ho made a number of observations to determine the pitch of the modified ending, and found that it is dependent upon the rate of movement of the end of the tube. A quick movement gives an "oop" higher in pitch than the tone; and a slower movement gives the same kind of sound, but its pitch is lower, and it is softer and has greater volume. Certain



rates of movement give a pitch that is equal to that of the generator. Both forks and variators were used as stimuli. If these observations, made under loose experimental conditions with hand-control of the speed of movement, may be trusted, it is possible that we have an explanation of the results in Table IX; for the judgment of pitch of the modified ending is correlated with the objective suddenness of the movement which interrupted the tones. The observations also furnish additional evidence that the modified ending may be objective;<sup>9</sup> and that, if a means of interrupting tones could be found which did not affect the air in the path of conduction, it might in so far be eliminated. It may, however, be impossible to deprive a tone of its normal means of propagation, the air, without at the same time setting up disturbances in the air.

Such results as ours call for further experiments conducted under the best possible conditions. As our problem was to demonstrate the presence or absence of an auditory positive after-image, the modified endings are incidental to our purpose; and it is unnecessary that we should make a more complete investigation of the conditions upon which they depend. Since, however, their occurrence, in so far as it represents an objective disturbance of stimulus at cessation, might give ground for the objection that a possible faint after-image has been covered up and overborne, and thus has escaped the notice of our O's, we have made some further experiments with tones of greater intensity and longer duration.

*Experimental: Complex Tones*

In order to extend our results to other kinds of stimuli and to long stimulation-times, we conducted a group of experiments with more complex tones. In some experiments the original stimulation-times were used, and in others the time was extended to 5 and 15 min. The tones were C' and C'' from organ pipes of metal; C and C' from organ pipes of wood; and tones number 2, 4, 6, 8, and 10 from an Appunn reedbox of the overtones of a fundamental of 64 vs.

Our O's in this group were Miss E. C. Comstock (C), graduate student in psychology; Mr. F. L. Dimmick (D), assistant in the department; H, Ho, M, S, T; Mrs. A. K. Whitchurch (Wh), graduate student in psychology; and a class of 7 students in the advanced laboratory course.

<sup>9</sup> It was noticeable from the very first that O's who were observing in the same group did not by any means necessarily report in the same way upon the same stimuli. We have already shown that they varied in their attitude, attending sometimes to the tone's ending and sometimes to the period immediately following. The objective or partially objective character of the modified ending is, therefore, not put in question by the variety of report. Moreover, as we have also shown above, prescribed direction of attention led to uniformity of report.

All tones were blown at full intensity, at the different durations. The *O*'s sat where they could see the *E*'s hand move as the tone was cut off. This movement replaced the signal flag of previous experiments. In all, 448 observations were made. Of these 11 were with 15 min., and 7 with 5 min. stimulation.

### *Results*

Not an after-image was reported; but our practised *O*'s heard modified endings, and felt pressure in the ear from long stimulation.

Our regular experiments had indicated that pressure in the ears made its appearance in place of an after-image, and we wanted striking evidence that this indication was true. If naïve *O*'s could be brought to sense the pressure, without practice, this result would be good evidence of its fundamental nature. Accordingly, our unpractised *O*'s were instructed to describe any after-effect of stimulation that might appear. They were then given number 2 on the reedbox as stimulus for 15 min. All of the group, except one who misunderstood the instruction, reported vivid, throbbing pressure which lasted from 5 to 10 min. after stimulation. Nothing was heard. One *O* remarked that there was no after-image, unless the throbbing were a negative after-image; and added that in this condition the ears "felt very sensitive to noise." If 15 min. of stimulation resulted in so perceptible a pressure, it may be supposed that the naïve *O*'s who were able to report it were also capable of perceiving any tonal after-effect, and that their failure to perceive tone is further evidence that no auditory after-image exists. The case of heightened sensitivity to noise may help to explain why, in the regular series of experiments, long stimuli led to more frequent report of modified endings.

### *Auditory Recurrent Images*

For *T*, the tone frequently recurred after stimulation. *S* reported only a single recurrence, the other *O*'s none. The attributive correlates of *T*'s recurrences are given in Table X.

The recurrences were generally 1 to 2 sec. in length, but sometimes were as long as 10 sec. More than a single recurrence was not reported after the sources of tone had been placed in sound-proof boxes. There were numerous 'recurrences,' as many as seven, when the purr of the motor could be heard through the conduction-tube. These recurrences are, of course, not trustworthy. As a rule, the recurrent tone had the same pitch as the stimulus; but it was sometimes one or two octaves above or below. Localization was generally

in the head, though sometimes recurrences were unlocalized. B found that, after serving as *E* for one or two hours, the recurrent tone came as an addition to any faint continuous objective sound, but that in a quiet room no recurrent image could be heard. Our recurrent images obviously resemble the "secondary" after-sensations of Urbantschitsch.

TABLE X

Series	INTENSITY			DURATION			PITCH			
	Str.	Med.	Wk.	30	15	5	1024	512	256	128
I	10	11	3	8	9	7	1	8	11	4
II	0	1	1	2	0	0	0	1	0	1
III	8	3	0	3	5	3	5	3	0	3
IV	5	1	1	4	2	1	435	350	256	100
Total	23	16	5	17	16	11	2	1	4	0

In Tables III and IV, T's results are often not in agreement with those of the other O's; but it will now be seen that the disagreement is due to the number of recurrences at higher and middle intensities, or in long and middle durations. The unequal distribution for pitch in Table X may be due, in part, to the influence of the register of T's voice; the recurrences are most frequent within his singing range.

## CONCLUSIONS

1. There is no positive after-image of tone, analogous to the positive after-image of vision.
2. The 'modified ending' of tones, which we have been unable to eliminate, is probably a compound effect, due in part to tonal *Abklingen*, in larger part to the objective conditions of our experimental arrangement.
3. Intensity of modified ending is dependent upon intensity of stimulus; its vividness or insistence upon pressure in the ear, which increases with intensity and duration of stimulation.
4. It is impossible from our experience to say which of our two sources of tone, the variators with air conduction or telephone receivers, is to be preferred. The variator tones were relatively weak; when they are cut off, slight noise accompanies the swinging of the conduction-tube; if the conduction-tube remains unbroken, echo is a source of error. They furnish, however, an almost purely tonal *Abklingen*. The telephone receivers give tones approximately equal in intensity at all pitches; in the period when the after-image is expected to appear, they are absolutely noiseless; there is no difficulty from echo; but the *Abklingen* is very impure, owing to an admixture of noise from the diaphragm.

# THE INFLUENCE OF COLOR UPON MENTAL AND MOTOR EFFICIENCY\*

By SIDNEY L. PRESSEY

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## I. PROBLEM

The problem of the study may be put as follows: Do different hues and brightnesses of general illumination have (*aside from* the obvious importance of these factors in conditioning the effective functioning of the visual apparatus) any specific influence upon mental or motor efficiency? The subject is obviously of great practical importance. If adequate lighting in factory or office tends to stimulate activity and increase the amount of work done, as well as to aid in visual efficiency, the matter is of no slight moment from a business point of view. If, as is often supposed, an office or

\* From the Psychological Laboratory of Harvard University. The paper is a summary of the writer's thesis for the doctorate, which was presented in April, 1917, but which, because of pressure of other work, it has not been possible until now (October, 1919) to condense and arrange for publication.

school-room furnished in red has a distinctive effect, upon mood and upon sustained capacity for work, as compared with a room furnished in light green or buff, then the definite demonstration of that fact would be of decided practical value. The aim of the present study was to investigate experimentally, in the laboratory, this question as to the effect of brightness and of hue upon mental and physical work.

## II. PREVIOUS WORK BEARING UPON THE SUBJECT

As thus conceived, the study differed, in certain important particulars, from other investigations in this general field. In the first place, color combinations were not considered. In the second place, large surfaces, involving the subject's entire field of vision, were used, not small stimulus-areas (usually the subject worked in a room illuminated solely by a light of the desired brightness or hue). In the third place, the interest was in objective measurements of mental work and neuro-muscular tone, not in introspective reports. Finally, as has already been mentioned, the study was not concerned with the effects of hue or brightness upon visual acuity or ocular fatigue; any such effects were carefully avoided. Previous studies in the aesthetics of color have, however, most important bearings upon the writer's problem. A consideration of these previous studies may be conveniently taken up under three heads, according as the work was (*a*) introspective or observational, (*b*) experimental, or (*c*) analytical and critical.

(*a*) *Introspective and Observational Studies.* The earlier work treats rather of the artistic values of the different colors than of the narrower problem of the relation of color to affection. It is impossible in brief space to summarize the mass of this material. It may be said, shortly, that introspective consideration of the matter seemed to indicate that each color conditioned a specific emotional and affective response of great richness.

Goethe's "Farbenlehre" is the most important early treatment. There are "active" and "passive" colors. The hues from yellow through red are exciting, lively, arousing, the climax being reached in the red. From blue back to red is the minus or passive side; and at green is a balance of arousing and depressing influence which gives a feeling of peace and comfort. There is thus a complex and intimate relation between color and affection.<sup>8</sup> Such theories find their best known and most systematic formulation in Wundt.<sup>40</sup>

Anecdotal data with regard to color,<sup>88</sup> anthropological observations,<sup>15</sup> and studies in the history of art,<sup>16</sup> may also



contribute to the problem. Savages, and also many animals, show a marked fondness for bright colors and for shiny, brilliant objects. As evidences of the same tendency in civilized man may be cited the fondness for diamonds and other gems, for fire-works, for lacquer, gilding, boot-blackening, and the adoption of shiny metal for money.<sup>15</sup> Among the hues red has an outstanding position; the power of red to attract attention and arouse action, in both men and animals, is made much of.<sup>10</sup> However, there may be striking differences in the affective character of a hue, from one locality to another or one period to another. Thus yellow was in classical times a favorite color, and is now least liked.<sup>11</sup>

(b) *Experimental Studies.* Midway between such observational studies and the laboratory research comes a mass of more or less roughly controlled investigations, mostly with children, of which the monographs of Miss Shinn,<sup>34</sup> Mrs. Moore,<sup>26</sup> Preyer,<sup>29</sup> and Major,<sup>24</sup> and the papers of Winch,<sup>39</sup> Wells,<sup>37</sup> Jastrow,<sup>18</sup> may be mentioned. The gist of the results may be put in a sentence. There is an early fondness for bright colors, decreasing with age; an especially rapid dropping back of yellow, ending in least liking; in general blue and red are liked best.

The laboratory work may be summarized quite as briefly. There is an almost absurd difference of opinion among many of the experimenters as to the affective value of the various hues. And experimental studies of special problems in the aesthetics of color show little more (relevant to the present problem) than the extreme complexity of many reactions to color, and the multitude of factors which may play a part.

Cohn<sup>7</sup> states that his is the first experimental study of the aesthetics of color. He placed his subjects in a dark chamber, and used for stimuli small gelatine plates; the illumination was by daylight. He found, briefly, that saturated colors were preferred, but that as between different hues of equal saturation the choice was a matter of individual idiosyncrasy. On the whole, yellow was least liked; but the data are not considered adequate for any generalization in the matter. Cohn had seven subjects, and worked by the method of paired comparisons. Major<sup>25</sup> used colored papers, and a different method. He had four subjects. Neither antipathy for yellow nor preference for saturation was discovered. Cohn<sup>8</sup> thereupon experimented further, and confirmed his previous results as to saturation. Miss Baker<sup>2</sup> found the warm end of the spectrum most pleasant. Fernberger<sup>13</sup> found relative dislike for yellow and yellow-green. Miss Washburn<sup>36</sup> found that thirty-five college girls liked tints best, shades next, and saturated colors least. Blue was preferred, of the hues.

Some few papers have appeared which try to deal experimentally with the apperceptive elements in color attitudes. Thus Bullough<sup>4</sup> worked on the question of the apparent heaviness of colors. He concluded that a color looks light or heavy according to its luminosity,

stressing this element rather than any association of dark and low objects, or analogous explanations. Pierce<sup>28</sup> found that bright colors "weigh" more than dark ones in making up an ornamental balance; he explains this on the basis of the "action theory" and greater demand of the bright objects for eye-movements. Quantz<sup>30</sup> and Languier des Bancel<sup>20</sup> agree that color affects the apparent size of objects. Thus red surfaces look larger than equal blue ones. But no satisfactory explanation is offered.

Investigation by the method of expression has led to similar contradictory results. Or rather, it may be said, that the best work of this type has given negative findings.

First to be mentioned of such studies should be Féré,<sup>12</sup> who, while experimenting with hysterics, found the most remarkable differential reactions to different tones, different tastes, different smells, and finally to different colors. If the patient was shown a red light there was an increase in pulse, a disturbance of breathing, a distinct increase in muscular strength as shown by the dynamometer. And, since he considered his neurotics to exhibit in marked degree what was true in kind of normal individuals, Féré concluded that, with normal individuals also, red had a dynamogenic effect.\* A Leipzig study by Stefanescu-Goanga<sup>35</sup> comes to a somewhat similar conclusion. Striking results, giving confirmation of the Wundtian theory of the correlation of bodily changes and tri-dimensional feeling, were obtained. However, McDougall<sup>28</sup> found no specific effect of either hue or brightness upon reaction time.† Angell and Thompson<sup>1</sup> found no evidence of any specific association between organic changes and various visual stimuli. Shepherd's very careful work also failed to yield any evidence of such a connection.<sup>33</sup>

(c) *Analytical and Critical Studies.* The great importance of social influences, of literary and religious symbolism, and of language, in developing affective reactions to color is the one point constantly stressed by the more critical writers. That other than intrinsic values are important is apparent from the way color preferences change in art, and even with famous artists during their lifetimes. The effect of a color may be totally changed by slight change of circumstances. And such affective toning, though the result of previous associations, need by no means show any trace of those associations. The ideas and feelings have grown together, amalga-

\*Féré's work is extremely interesting. But surely these hysterics, who present as perhaps the fundamental symptom an extreme suggestibility, are exactly the persons who would respond most completely to the many subtle suggestions of language, art, and custom, which will shortly be mentioned (not to speak of possible unconscious suggestion from Féré himself). For the distinguishing of a true dynamogenesis, as distinct from the effect of such influences, no subjects could be less reliable.

†Prof. E. B. Holt has told the writer of a very similar study, made by Holt, with completely negative results. The average times, with the different hues, were almost perfectly identical.

mated their effects, until these are indistinguishable from innate hereditary attitudes.

So Müller-Freienfels<sup>27</sup> emphasizes the extent to which a color may be given an affective value simply by its name, as blood-red, or violet, or lilac, or orange, or lavender. The red-furnished room mentioned by Miss Calinich<sup>6</sup> is also of some interest. If the room was warm the reds seemed bright, lively, cheerful, warm and enlivening. But if the room was cold these same reds were dreary, depressing, with a peculiar, dead, chill effect.

The odd artistic career of yellow may also be instanced to show the extent to which even very special and artificial circumstances may have a profound effect upon the esteem with which a color is regarded. Yellow was a favorite color in classical times, and is now in the East. But there is, as has already been mentioned, some evidence to indicate that for the average European it is the color least liked. The change is (according to Havelock Ellis) due to the Church; the early church authorities looked upon the color with disfavor because of its association with pagan festivals and licentiousness, and succeeded in building up about the hue a group of symbolisms and associations of the most unpleasant character.<sup>11</sup>

Previous work bearing upon the writer's problem may, then, be briefly summarized as follows. (a) Introspective and observational studies show colors, as they appear in everyday experience, to have a marvelous richness and complexity of affective significance. There is the suggestion that color may be a conditioning factor of distinct importance in influencing the efficiency of mental work. (b) Laboratory experimentation for the most part fails to find any constant relationship between color and either introspective evaluation or organic reaction. (c) Analytical and critical studies emphasize the importance of artistic conventions, symbolism, language, and everyday association of certain colors to other sensations affectively toned, in giving an emotional connotation to the hues and brightnesses.

It would then seem reasonable to conclude that if color *does* have any fundamental physiological effect, such as would influence mental and motor efficiency, the connection must be of a very general and elementary nature; brightness may stimulate, or red irritate and distract, but more specific effects are hardly to be expected. It is some such relationship as this, between illumination and mental work, which is sought in the writer's experimentation.

### III. EXPERIMENTAL RESULTS

#### A. *Preliminary Experimental Definition of the Problem*

This experimentation began with a brief preliminary investigation, of an exploratory character, in which large sheets of colored paper were used. Nothing more need be said about

the method than that the subject was seated comfortably at a table, with a large sheet of colored paper arranged in front of him so as to be almost the only object within his field of vision. While in this situation, thus dominated visually by the color, the subject was asked to introspect, and to do certain tests. Nothing more was done with the tests than to make trial of them. But the introspection yielded some points of interest. In the first place, the finish and texture of the paper appeared exceedingly important, especially so far as pleasantness and hold on the attention were concerned; the "energizing" or "stimulating" effect seemed more dependent on the hue. Where there is a shimmer, or where the light falls across a rough finish so as to emphasize the roughnesses, the result is peculiarly disagreeable. Any smudges on the paper were sure to be noticed, and were likely to play a part in the association; thus the dominant feeling of one subject for an orange was uneasiness centering in the kinaesthesia of a desire to reach out and smooth an unevenness in the paper. In the second place, the introspection suggested that the special affective values often assigned to such colors as lavender, purple, orange might be due to other causes than the hue itself. Such colors are unusual, and so attract and hold the attention; because they are unusual, what associations they have are definite and concrete, and give great vividness. With repetition, however, these factors usually drop out, and the color is ranked along with the primaries, as more or less bright, stimulating, depressing; is sometimes liked merely because it has in it more or less yellow or blue. Finally, individual differences, both in the evaluation of the different colors and in the general type of reaction to them, were marked. In fact all of the types mentioned by Bullough<sup>5</sup> and Bradford,<sup>3</sup> with variations, appeared at one time or another, from crude sensory effect to complex personalizing attitude. The reports made excellent evidence for the complexity of responses to color mentioned in the preceding section.

Thus one subject reported of a striking reddish purple that he "liked the color. But there was no special effect, aside from the pleasantness; otherwise it was neutral." And at the end of the color period it was "about as before. No great fatigue. The affective value was somewhat less, but fairly durable." Very different are the descriptions of another man, who says of a rough-finished yellow that it is "heavy, like an overcoat; queer feeling; not pleasant, not unpleasant. Impression that color is deep, and thick. Kinaesthetic sensation in finger tips as result of thickness. Heaviness seems referred largely to texture." A light yellow green is "very pleasant, a buoyant feeling." Another day this same subject finds the same color "depressing; the color of grass when it's dying."



More explicitly associative is a report on yellow. "A slight greenish effect; brought up idea of grapefruit tree at the Exposition and emotions going with that trip. Now wonders how got green; had seen stripes of green." An orange was liked; but then, with a thought of the war, came an idea of this as the color of bloody water, and a feeling of repugnance.

Different still in type of response is the characterization of a red as "too aggressive; it pushes itself upon you, won't let you alone, rude. I don't like it."

Enough has already been said to suggest the unsatisfactoriness of colored papers as stimulus areas. In the first place, no adequate control of brightness was possible; but such control was clearly essential if an adequate analysis of the problem was to be made. In the second place, the texture of the papers, and any wrinkles or spots, played too important a part in the subject's reaction to make adequate control possible. And in the third place it was not feasible to arrange the experimental setting, with colored papers, as was necessary for the problem. The problem was to determine the effect of color, as a dominant but unobtrusive and natural element in a situation, on mental and physiological processes. If paper were to be used little less than to have the walls covered with it would suffice. Under the circumstances, trial was naturally made of colored lights.

With regard to the preliminary work with colored lights, nothing more need be said than that it was of the same general character as the systematic experimentation to be described shortly. Work with the colored lights was begun in March (1915) and the remainder of that school year was spent simply in trial of various tests, light screens, and so on. The results to be described in the next section were, therefore, obtained with methods which had been carefully elaborated in this trial series.

The preliminary work served chiefly to develop methods and setting and to make trial of tests. The introspective material may be summarized as follows. (1) Affective reaction to colors is highly variable, both from individual to individual, and from week to week with the same individual. No hue, except perhaps red, may be said to show characteristics of any constancy. (2) The reaction is very easily modified, or often wholly changed, by (a) subjective factors such as general condition, mood, constellation of ideas, or chance association, and by (b) incidental objective factors such as unevennesses in the colored surface, and especially the texture of this surface and the way in which the light is reflected from it (softly, or with a shimmer, glint, or glare). (3)



The reaction may be of all degrees of complexity, from simple sensory effect to elaborate associative development or subtle personalizing attitude.

Introspective treatment of the subject will be returned to, briefly, later.

#### B. *General Experimental Setting and Method*

The experimental setting developed during the last part of the preliminary work was continued, essentially unchanged, throughout the remainder of the study. It may be described as follows. One end of a long narrow dark room was shut off by a heavy black curtain, making a smaller room about 9x6 ft. Backed up against the curtain was a table three by five. Behind this, that is, facing the curtain and with his back to the end wall of the room, sat the subject. The table was covered with heavy unglazed drawing paper. Over the center of the table, and 26 in. above its surface, was the light-box (8 by 10 by 12 in.), held by a frame-work from the ceiling. This contained four 100 c. p. Mazdas. In the base of the box were slots, where the colored plates could be slipped in. The light box was dead black outside, but lined with metal to reflect the light downward; on the side of the box next to the subject was a black pasteboard drop hanging just low enough so that none of the direct light came into his face. At one end of the table, and dropping down almost to its surface, was the cord, with its glove finger, for the tapping test. Well off to the other side was the small block of wood with the two keys, for the reaction test; this was hooded over with a piece of the white drawing paper, to make it as unobtrusive as possible. The glove finger and the reaction key block were the only pieces of apparatus visible to the subject.

As has been said, none of the direct light came into the subject's face. The light shone directly, however, on almost the whole surface of the table, but very little beyond this. The situation was thus not at all uncomfortable for the subject. He sat easily at the table; the light came from a position not uncommon in ordinary lighting arrangements. There was no shimmer, glint, or glare, from the unglazed paper, only a soft, diffused, indirect illumination. And aside from this there was only the hardly-to-be-seen blackness of walls and curtain. This was the situation when the test lights were burning; when the "normal" light (a 16 c. p. Mazda fastened to the light-box on the side away from the subject) was on, between tests, the situation was very little changed. The lighting was still indirect, came from the same position approximately, and was of roughly the same brightness as the colors used.

On the other side of the curtain, and against the wall, was the experimenter's table. On this were the various recording devices, the theatre dimmer for equating the brightnesses of the colored lights, the time keys, and so on. Here also was the double-throw switch, arranged to throw on the test lights at the same instant that the "normal" was thrown off (and the reverse), so that there should be no period of darkness, or irregular interval, between the two stimuli. The experimenter's table was lit by a four candle-power lamp in a hooded "goose-neck" holder; it was wholly invisible to the subject behind his heavy curtain.

From now on the study consisted of two separate pieces of experimentation, making use, however, of the same tests, the same methods and the same setting; the "hue series" was planned to discover any possible effects of hue, independent of brightness, and the "brightness series" to isolate any influence of brightness, upon mental work. The plates used in the hue series were complementary blue, pure green, red. The stimulus lights were equated for brightness (by episicotister) in terms of the darkest, blue; to do this a theatre dimmer was used to shift the illuminating power of the lamps according to the absorption of the different plates.\* The illumination, on the top of the table under the lamps, was about seven candle-meters. The brightness series with which most work was done was also a three-unit series. The dim light was obtained by stepping down the current with the dimmer and putting in the white screen; it gave an illumination on the surface of the table of about one candle-meter. The medium light was given by a single 32 c. p. Mazda, placed in among the larger lamps in the box so as to give the same distribution of light, and thrown in by double-throw switch after the "normal" in the same way. The bright light finally settled upon was the full power of the four 100 c. p. Mazdas.

For one semester, however, a five-hue and a four-brightness series were run. The five-hue series consisted of the hues above mentioned *plus* yellow and white. For white a special plate, a double thickness of "pot-opal" glass, was made; at the above-mentioned brightness the light through this was found to be almost pure physiological white. The four-brightness series consisted of the same two extremes as were used in the three-brightness series, but with the white of the hue series, and a medium bright light made by the white plate with the full power of the four Mazdas behind it, in place of the 32 c. p. "medium."

Three possible criticisms of this choice of stimuli remain for brief discussion. In the first place, it may be said that an effort should have been made to obtain monochromatic colors for the hue series. To

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\*The writer is indebted to Dr. L. T. Troland for the equating of the brightnesses, and for the rough analysis of the lights here given.

this it may be replied shortly that the problem of the study was essentially a problem in applied psychology, and that what was sought was the effect of colors such as might be used in actual schemes of illumination or decoration. The important requirement was that the lights should be seen by the subjects as red, green, and blue, of a not unusual composition or saturation, and that the saturations should be approximately equal. The lights were, as a matter of fact, almost invariably judged by the subject as simply blue, green, or red without admixture of other hues; and they were considered about equally saturated (the yellow was dropped partly because less saturated). Nothing more seemed essential. Monochromatic light, supposing it could have been obtained for use over such areas, would have been a highly artificial form of stimulus much less relevant to the practical problem.

It may be objected, in the second place, that more hues (and more brightnesses) should have been used. The difficulty was mainly a matter of method, and of experimental practicability. Most of the subjects came to the experiment only once a week, for a 50 min. period; five or six colors meant only seven or eight minutes with each. The problem, however, was to determine, not the effect of the presentation of the color, but the influence of its continued presence as part of a work situation. Longer periods were thus clearly necessary. So it seemed best, for this and various other reasons, to limit the experimentation to intensive study of three hues, and three brightnesses. Practically all the previous work (not to mention the writer's own preliminary study) indicated the colors at the two ends of the spectrum as the hues most diverse, fundamentally, in their effects. It seemed reasonable to suppose, therefore, that an intensive study of red, of green, and of blue would indicate with fair adequacy the direction and amount of the influence, if any were to be found, and give the best evidence for negative conclusions in case the results were negative.

In the third place, criticism might be made of the "very bright" light of the brightness series, as too strong, possibly so bright as to trouble the eyes, and so introduce factors not relevant to the problem. The writer expected such difficulties. And he was much surprised to find, after extended trial with all the subjects, that in most instances this bright light was the most pleasant of the series! The subject received only indirect, diffused light; there was no glint, glare, or other unpleasant distribution. Under these circumstances this light was usually introspected upon as stimulating, cheering, very pleasant. And anything less bright failed to give the desired distinctness from the "medium" light.

As has already been intimated, most of the work was done with three hues, or three brightnesses, to the hour. This permitted 12 to 15 min. with each color, with intermissions between colors of about 4 min. The colors were given in a definite routine; if, for instance, subject "A" was given on the first day red first, green next and blue last, then for his next hour (the following week) he was given green, blue, red, and the third hour blue, red, green. After three hours, then, each color had appeared once in each position in the hour. This made up a unit, within which the various factors of the work curve (fatigue, practice, and so on) were evenly

distributed among the colors. The next group of three would begin red, blue, green: in this way every possible order was tried. Between colors the "normal" light already mentioned illuminated the subject's room.

The number of subjects varied from one semester to another; usually there were six or eight, and two came twice a week, the rest once. Subjects who came twice a week were given one hour with the hue, and the other hour with the brightness series; the other subjects were divided up equally between the two series. Regular hours were assigned; the experimentation was done in the morning, in hour periods, between nine and one. The subjects were all trained observers, and familiar with laboratory methods and technique.

In the entire study (including the preliminary work) twenty-six persons at one time or another acted as subjects. One was a professor of psychology; seventeen were graduate students with experience in research. Eight were Harvard undergraduates doing advanced work in psychology; three of these were carrying on independent research. Three of the graduate students were women. It should, perhaps, be added that the study was made during the university years 1914-1915, 1915-1916, and 1916-1917 to the first of March.

Much difficulty was found in securing satisfactory tests, because of the fact that the purpose of the experiment prevented the employment of any test requiring use of the eyes; the purpose was not to study visual acuity with lights of different colors, or the effects of eye strain in dim light on mental work, but to discover whether *apart from such factors* different hues and different brightness had any influence on general neural tone and mental efficiency. The vast majority of tests previously used in such comparative studies (studies of the effects of drugs, for instance) were thus barred from the writer's experimentation. And much time was spent in simply developing tests which would meet these special requirements. The tests finally adopted will be described in detail later, along with the presentation of the results obtained. A large number of other tests (as dynamometer measurements, maximum speed in tapping, several forms of memory test) were tried, but found unsatisfactory and rejected.

The test routine varied, of course, with the tests employed. The first of the work was done with the following tests, and in the following order. First, under the normal light, the subject tapped for 30 sec. (this was not the usual tapping test, but a tapping "at most comfortable rate," as will be described later). Then the normal light was thrown off and the test color on; the tapping, however, continued for another 45 sec. Introspection was now called for. After this, the multiplication was given, then the pressure test, then the memorizing. Forty-five sec. more of tapping followed this. Finally, introspection



was asked as to any change in the influence of the color from that reported when the light first appeared. The normal light was now thrown in, and the subject rested for 4 min. while the experimenter changed the colored plates, adjusted the theatre dimmer, slipped new smoked papers on the kymograph drums and adjusted the recording apparatus. Then the same programme was repeated with the other two hues. Introspection was called for only during the first two or three weeks at the beginning and end of a half year's work; for the greater part of the time the work was wholly objective.

This general arrangement was adhered to throughout the experimentation. As sufficient data were accumulated with a test it was dropped, and a new test substituted; the routine, however, remained essentially the same. Work in the color series and in the brightness series was also throughout the same; there were the same conditions, the same tests, and the same routine; so that the two series were in all respects strictly comparable.

Every half year saw a change in programme of some sort. With a new test three hours of practice were allowed, as a rule, before results were used. Data from twelve hours following this were the aim. This was not always possible, however. By the methods used, the results with a given color, in a given hour, and on a given test, obtained their significance from comparison with the results, on that test, under *all* the other lights. If now, reaction times under green were lengthened by the sound of the bell at the end of the hour then all the results, with the other colors as well, were warped in their significance. So the rule was made that no results from any hour which was interrupted, in any way, on any test, should be included. A large amount of material was, of course, eliminated by this rule; but the eliminations seemed necessary, if the subtle effects sought were not to be buried completely under such chance variations. The endeavor was also made to obtain, in each series, results from at least six individuals, on each test. This was, again, not always possible (or worth while) but was done in the majority of cases.

A word remains to be said regarding the handling and interpretation of the data. In the investigation of any such subtle influences as were being sought chance variations in the results are certain to be large, and differences (if any) due to the factors studied, small. Satisfactory interpretation and evaluation of the data are thus extremely difficult. The situation is, essentially, too complex to make possible any summary of the consistency of the findings, which could be treated statistically. Instead, a judgment upon the reliability of the data has been attempted, based upon three related, but distinct, sets of facts: (1) the consistency of the results obtained from each subject, on each single test; (2) the degree of agreement among different subjects on each test; and (3) the consistency of the results from test to test with each subject (a correlative consideration here being, of course, the consistency of the group from test to test). Any final numerical statement of the reliability of the findings must, of necessity, be partial and inadequate. Instead, an attempt has been



made to bring all the facts, considered from these three points of view, together in a judgment as to the probable meaning of the data.

The writer is not at all sure, now (1919), that a more determinate and decisive statistical treatment of the material would not have been possible. But, in defence of the procedure adopted, it may be pointed out that practically all the well-known studies having similar statistical problems have been quite as indecisive in their statistical statement. Most similar, in the statistical problem presented, are studies of the effects of drugs, of which the monographs of Hollingworth<sup>17</sup> and Dodge<sup>9</sup> may be cited. And it might be pointed out in this connection that, so far as difficulties of control and analysis are concerned, study of the effect of only three colors, in one short hour, is not a little analogous to an attempt to study the effect of three drugs in that brief space of time. The problem was not an easy one, and abounded in unusual difficulties of analysis and statistical treatment.

The methods used will be discussed in more detail in considering the results obtained with each test.

### C. Results

1. *The Effect of Color upon Rate of Rhythmic Movement of the Finger.* This test was planned to give an indication of any change in neuro-muscular tone and tendency to movement. It was, essentially, an ergograph with minimal resistance. Clamped to the end of the table was a rod which reached obliquely in over the table top. The end was about 15 in. above the table, and the same distance in from the table end. To the rod was fastened a pulley. Through this ran a silk cord, to which was sewed a glove finger. The subject put his left fore-finger in the glove, and rested his arm easily on a support which kept his finger free above the surface of the table. The test consisted simply in moving the finger up and down with a tapping movement. The white silk cord ran over to the experimenters' table, where it was fastened to a marker which wrote on a smoked drum, and to the resistance, a single piece of ordinary rubber band. The subject was told to tap at an easy, comfortable rate, which could be kept up without fatigue during the minute tapping period; to find the rate which was most natural and comfortable for him; then to forget all about the test, letting the movement go on automatically. There was a specific caution against any effort at speed.

The test should, the writer felt, be quite highly sensitive to any energizing or stimulating effect the colors might have. If red *did* excite, or a dim light subdue, this ought to show easily in such a half-conscious rhythmic activity as this simple movement, up and down, of the finger. But there were other advantages. Practice effects were unimportant; there was no learning; there should be nothing of those

wide variations likely to appear when there is any requirement of maximal effort.

The place of the test in the routine has already been described. The tapping was begun 30 sec. before the stimulus light was turned on, and was continued for 45 sec. after this. The other tests were then given. And finally, just before the stimulus light was turned off, there was tapping again for another 45 sec. Time was marked off in 15 sec. units. The results were handled in terms of these units. The number of taps in the first two quarter-minutes under the normal light were averaged to give the average normal for the period. The number of taps in each one of the six quarter-minutes under the stimulus light were then scored so much *plus* or *minus* from this normal for the period. And the difference between the variation from its normal produced by red, and the variation from its normal produced by the blue, gave the difference in effect of these two colors.

This final figure, it should be emphasized, was almost wholly free from the influence of constant errors. With some of the tests, systematic variation through the hour, due chiefly to difficulty in getting started with the work, and to fatigue in the last period, were considerable. However, with this arrangement of the tapping, the normal, for each period, just preceded the color period itself. If red came last in the hour on a given day, its normal came near the last too; the normal took account of the variations through the hour (as well as of the daily variations), and none of these irrelevant factors got itself into the results.

In handling the results the number of taps, in each 15 sec. under the color, was first scored as so much *plus* or *minus* from the average number of taps per 15 sec. under the normal light, as has been just described. The variations for the first three quarter-minutes under the color were then added, giving the total variation for the first 45 sec.; and the variations for the last three quarter-minutes also added. For instance, suppose the number of taps per 15 sec. ran as follows:

Green									
normal	first 45 sec.						last 45 sec.		
17 15	16	17	19	(other tests)			17	19	20

This would be scored

16	0	1	3	(4)	1	3	4	(8)
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To obtain the final percentage statement, the average normal was multiplied by three, to give the average normal for 45 sec. and the succeeding variations per 45 sec. stated as per cents of this. The above record would thus become

Green									
	normal	first 45 sec.						second 45 sec.	
	48	4						8	
and in per cents	48	8%						17%	

This could, of course, have been done less elaborately by running a normal of 45 sec. instead of 30 sec. But since the work was begun with a shorter normal it was so continued, and the above method of handling used.

		I		II		III		IV	
	Av. Nor.	1st 45° av. m.v.	Last av. m.v.	1st 45° av. m.v.	Last av. m.v.	1st 45° av. m.v.	Last av. m.v.	1st 45° av. m.v.	Last av. m.v.
Sub.									
And.....	46	5	6	4	0	3	6	2	10
Dav.....	55	3	13	3	3	0	2	4	9
Fin.....	49	8	6	11	—	5	3	—	10
Kal.....	102	7	6	7	10	2	7	9	20
Average (Ten hours each subject.)	..	.5	5	6	3	-2.5	4	5	12

Sub.	Av. Nor.	I		II		III			
		1st 45° av. m.v.	Last av. m.v.	1st 45° av. m.v.	Last av. m.v.	1st 45° av. m.v.	Last av. m.v.		
And.	62	16	7	18	12	2	5	13	9
Cur.	68	4	5	9	6	6	3	2	8
Day.	77	1	8	9	4	0	3	6	10
Woolb.	51	1	5	0	7	3	3	4	7
Mor.	124	6	7	12	8	11	10	16	11
Average.....	...	7	6	10	7	4	5	7	9

(For each subject in order the hours were 20, 10, 8, 9, 9.)



The results for the brightness series are given in the following table (Table I). They are stated, as has just been described, as per cent variation, from average under normal light for 45 sec., for 45 sec. under the color. The first series was run with four lights, bright, moderately bright, medium and dim. In this series were four subjects; the number of hours for each was ten. In the second series three brightnesses were used; the first and the last were the same as in the first series, but in place of the moderately bright and medium light a new medium, the 32 c. p. Mazda, was substituted. In this series were five subjects, two of whom had also been in the previous series. The number of hours with one subject was twenty, with one ten, with one eight, and with two nine.

The results may be summarized as follows. Of seven subjects five, including those from whom there are most results, show an increase in rate of tapping with bright light, and a decrease in dim light. One subject showed a slight, the other a more considerable, reversal of these results. In the case of this last subject, there is some evidence to suggest a special, somewhat morbid, peculiarity in this respect. For those subjects giving positive findings there was not only an increase under bright light, but a decrease under the dim light as compared with rate under the medium light. There was also no evidence of decrease of these positive findings through a semester; one subject, kept at the test for a year and a half with a view to obtaining evidence on this question, gave as positive results at the last as at the beginning.

It must be emphasized that the results from the two tapping periods, the first 45 sec. and the last 45 sec., have a very different significance as regards the possible effect of the lights. Any change in the first period, when the light had just been thrown on, might be due simply to a shift of attention, or some similar transitory element in the situation. The results from the last 45 sec. should be of much more importance, since the stimulus light has been on, by that time, some 12 min., and any effect then observed must be of some permanence.

As a matter of fact, the results are most distinctive in this last period. The group averages show greatest number of taps for the brightest light, and a regular and even decrease down to the least number for the dimmest light. When the groups are analyzed it appears, in the first place, that the two subjects who worked in both series agree substantially, in the indications they give under the two somewhat different conditions. In the first group of four (with four lights) all the subjects agree in showing most rapid tapping with the bright light, slowest with the dim. In the second group of five, three subjects give this same result. One, however, shows a slight, the other a fairly distinct increase with the dim light over the bright one. In this last case, introspection may throw some light on the situation. This subject (Mor.) was extremely fond of dim lighting. This was



due, he said, to a general liking for subdued light effects. He was fond of sitting in his room in the dark, or with the lights low, was very fond of candle-light, could think best when the lighting was dim. With the other subject giving opposed results there is no such clear-cut preference, though he did say once or twice that he could think better in the dark.

The results with the hues may be very briefly summarized. No differential effect of different hues, at equal brightnesses, upon rate of tapping appears. Two series were run, one consisting of red, yellow, green, blue, white, the other of red, green, blue. Eight subjects took part, five in the first series, four of these and three more in the second. It may be said, shortly, that no consistent findings appear with regard to any one of the hues studied. (See Table I.)

2. *The Effect of Color upon (a) Pulse and Respiration, (b) Estimates of Pressure, and (c) Judgments of the Pleasantness of Touch Substances.* It may be said at once that the work with pulse and respiration gave results which were entirely negative. The work was very carefully done; records were taken both at the beginning and the end of the color period, and studied with reference to both rate and shape of curve. But nothing appeared which could be correlated, in any way, with either brightness or hue.

The experiment of Stefanescu-Goanga,<sup>85</sup> already mentioned, was also largely repeated, with the writer's setting, but with negative results.

These negative findings are not, of course, proof positive of lack of any effect of hue or brightness upon the physiological processes. There may be an effect, and of a cumulative importance, on respiration and pulse. But the changes under such conditions must be, within the time any convenient measurement might extend (30, 45, or even 60 sec.) so minute as to be probably below the threshold of even the best technique.

It may also be said, with equal brevity, that results with the "pressure" and "pleasantness" tests were similarly negative. No influence of either brightness or hue, upon performance with these two tests, could be discovered.

The apparatus for the "pressure" test was simply an ordinary postage scale. For 1 min., during the 5 min. intermission between colors, the subject practiced pressing down exactly 16 oz., while looking at the dial. The scale was then taken away, but presented again, when the time during the color period came for the test; the subject was now given five trials at pressing down 16 oz. as previously practiced, but this time with the dial turned away from him. The record taken was the amount actually pressed at each attempt (the amounts pressed in the five trials being summed to make the "pressure" score for each color).

Just what function the test might be supposed to sample was not clear. Perhaps a bright light, or a red, if stimulating, would bring

about a more energetic push on the scale, and an overestimate. And a dim light might bring about less activity. But the fact was that no such relationships appeared. The test was interesting, but of no value, at least for the writer's experiment.

The experimentation with the touch substances was almost as simple and direct. A large variety of touch substances (different kinds of cloth, wood, metal, and so on) were fastened to small blocks of wood (one and one-fourth by two in. and one-half in. thick). Twenty of these blocks were put in a shallow box just big enough to hold them, five in a row with four rows. This box was covered with a hood of the heavy, unglazed drawing paper. When the time came for this test, the box was put on the table in front of the subject who, with his right hand, beginning at the upper left-hand corner, worked across the top row to the right. Each substance was felt in turn, and its pleasantness expressed on a scale of seven, where one was most unpleasant, four indifferent, and seven most pleasant. After the top row was finished, the subject dropped his hand to the next row, working from right to left; and so on till the twenty substances were all judged. The hood hid the substances from view, so that the subject had only a touch acquaintance with the materials.

After the test was finished the box was taken from the table, and the other tests given. During the intermission between color periods the experimenter, by a simple arrangement, completely and systematically changed the order of the substances. And from week to week the substances used were changed. The purpose in each case was to break up any memory of previous judgments on a particular substance, to make each estimate a product simply of the two present factors, the touch substance as it felt now, and the present affective attitude caused by the color. The results were handled by simply adding the values assigned to the twenty substances under each color, giving a total which might, perhaps, be called the affective level under the given light. The test seemed to the writer to have possibilities. But the findings were altogether negative. He still feels, though, that something of this sort might yield interesting data in a study of some other problem, to which it was better adapted.

3. *The Effect of Color upon Rate of Multiplying.* No study of this type would be complete without some sort of arithmetical test. Such tests seemed almost impossible of control sufficient for the purposes of the present problem. Little help could be obtained from previous work, since practically all tests previously used were ruled out, from the start, by the requirement that there should be no visual presentation. Nevertheless, the writer felt that an adequate study of his problem required at least an attempt at measurement of possible effects of color upon some such relatively complex form of mental work.

The test, as finally made up, consisted of multiplications of one-place by two-place numbers. Eleven such multiplications were given with each color; a test sheet, for a given day, thus consisted of three rows each having eleven problems. Three such sheets were prepared, enough for three weeks (or

a single series, where the colors came each once first in the hour). The three were then repeated in order. But the order of the colors was varied, so that any difference in difficulty of the different groups of eleven was distributed. The record was in time for each multiplication, as taken with a stop-watch, and in errors. In handling the material, that multiplication of the eleven which had the longest time was struck out; quite frequently a time would run much beyond the average as a result of some slight distraction; an elimination of the longest time of each eleven allowed, to some extent, for these variations. The times for the remaining ten were summed.

TABLE II  
TIME IN MULTIPLYING (BRIGHTNESS SERIES) IN TERMS OF % OF AVERAGE TIME FOR HOURS

<i>Four Brightness Series</i>								
Subject	I. (bright)		II.		III.		IV. (dim)	
	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And.....	89	13.0	101	13.0	106	13.0	103	8.0
Dav.....	96	5.6	106	9.5	101	7.8	97	9.8
Fin.....	97	3.5	99	7.5	104	9.0	99	8.0
Kal.....	99	9.0	94	11.0	97	5.0	109	6.0
Average.....	95	7.7	100	10.2	102	8.7	102	7.9
(Ten hours each subject.)								

<i>Three Brightness Series</i>							
Subject	I.		II.		III.		
	av.	m.v.	av.	m.v.	av.	m.v.	
And.....	100	6.2	99	4.7	101	5.6	
Cur.....	95	7.6	101	10.0	104	7.6	
Dav.....	99	4.9	99	4.8	102	5.3	
Woolb.....	97	7.0	102	3.9	100	6.3	
Mor.....	100	9.5	100	9.3	100	10.1	
Average.....	98	7.0	100	6.5	101	6.9	
(For each subject in order the number of hours is 10, 10, 8, 9, 9.)							

TIME IN MULTIPLYING (HUE SERIES). IN TERMS OF % OF AVERAGE TIME FOR HOUR

Subject	Red		Green		Blue	
	av.	m.v.	av.	m.v.	av.	m.v.
Allp.....	99	4.7	101	6.2	99	8.2
La.....	101	4.4	102	4.5	97	2.7
Bul.....	100	8.3	99	7.9	100	8.8
Br.....	103	6.4	98	5.6	99	3.6
Cha.....	100	6.5	98	5.8	101	10.2
Woolb.....	98	7.5	104	7.8	97	12.0
Tul.....	103	4.4	102	6.1	95	6.0
Average.....	101	6.0	101	6.2	98	7.3

(Twelve hours three subjects, nine hours four subjects. m.v. after the group average is, for each table, the average of the m.v.'s, not the m.v. of the averages.)

The sets of eleven were made up to be as equivalent as possible. Any three numbers, as 6, 7, 9, can obviously be combined to form six problems of the type used: 6 times 79, 6 times 97, 7 times 69, 7 times 96, and so on. Five and a half such groups of six would, therefore, make up the materials for a day's test sheet: eleven problems with each of three colors. The problems were distributed among the three colors to give as great variety as possible, and as great uniformity: if 7 was used twice with a color as multiplier those two multiplications did not come together, and there were two sevens as multipliers with each of the other colors. In giving the test, the experimenter simply read each problem to the subject, slowly and distinctly, as "six times eighty-seven." The subject did the work mentally, and then replied with the answer, "five twenty-two." And the experimenter took time from the last syllable he said in giving the problem to the last syllable of the subject's reply.

In combining the results for a half year or more, two important factors had to be taken into account: (1) variations within the hour, and (2) variations from one experimental hour to another through the year. In dealing with practice through the year, some percentage statement of ranking of the colors, within the hour, was naturally suggested. The method finally adopted employed, as norm for the hour, the average of work under all three colors. The time under each color was then rated as a per cent of this.

For instance, suppose on a given day subject A's time for the ten multiplications, under each one of the three colors, ran as follows:

red	green	blue
55.0	57.2	59.2

these three would be averaged, giving 57.1., and per cents taken from this making a score of

red	green	blue
96%	100%	104%

Averages of these per cents through the year gave results free from practice effects from hour to hour. They were free from the many other variations from week to week, due to changes in condition, previous fatigue, and so on. Finally, the results from individual to individual were directly comparable; the average actual time for some individuals was over twice that for others.

The results are summarized in Table II. Mor., who gave results contrary to the tendency of the group in the tapping, does the same here. Otherwise there is a fairly consistent slowing with dim light, and acceleration of work with the bright.

The hue series gave no results so consistent. There is a suggestion of most rapid work with blue and slowest with green. But inspection of the individual averages reveals no consistent tendency such as appears with the brightnesses; minimum rate occurs three times in green, twice in red, once under blue, red and blue are once "tied for first place."



4. *The Effect of Color upon (a) Rate of Free Association, and (b) Immediate Memory for Nonsense Syllables.* The arithmetical work called for associational processes of a rigidly controlled and limited sort. As a contrast to this, it seemed interesting to try what the effect of color might be upon wholly uncontrolled, free association. Some test of memory also seemed necessary for a rounded test programme. Memory for nonsense syllables and rate in free association, under the various hues and brightnesses, were therefore investigated. And, it may be said at once, the results were entirely negative.

The materials and method for the free association test were substantially the same as used by Professor Langfeld in his study of the effects of prolonged fasting.<sup>10</sup> His lists of words, twenty to a list, were used, twenty words with each color. Timing was with a stopwatch. The experimenter simply read each word, slowly and distinctly, to the subject, who was instructed to reply at once with the first word that came into his mind. Time was from the last syllable of the stimulus word to the last syllable of the response. In handling the results, the longest and the shortest times were struck out, as most likely to contain extreme chance variations, the longest as perhaps due to a complex, the shortest as due, perhaps, to a mere perseveration from some previous word or associate. The total for the remaining eighteen words was then found.

The memory test was quite as simple. Immediate memory was clearly the only type which could be controlled adequately for the purposes of the experiment. Various kinds of material were tried; nonsense syllables were found to be the best suited to the test programme. Ten syllables were used with each color; a sample list runs as follows:

zok dib niv bam zek miz niz dop lor tem

In making the lists no long vowels were used (since these were found to "cling" in the memory more), and various other more usual precautions were taken, to make the lists of even and equivalent difficulty.

In giving the syllables they were read at a rate of two per sec., with a grouping in pairs by inflection of the voice. After the first reading the subject was asked to "give back" as many as he could. The list was then read to him again, and he was again asked to repeat all he could remember. This was done twice more, making four readings in all with each list. The syllables were scored as correct or half correct, in the recall; the final record was the sum of the recalls after each of the four readings. For instance, under red, the number of syllables given back after the first reading might be four, after the second five and a half, after the third seven, after the last eight. The record under the red would then be 24.5. The test was a great bore to the subjects, and yielded results of no value.

5. *The Effect of Color upon Rate of Continuous Choice Reaction.* A measurement frequently obtained, in work of this general nature, is speed in choice reaction. The writer felt strongly, however, that the classical reaction-time experi-



ment was neither suited for inclusion in his test programme nor calculated to get at the effects which were being sought.

It was at first planned to use the standard method, two stimuli and two keys, and the time in sigma for each separate reaction. But the technique was considerable; much time would be required to accumulate a small amount of data; and the measurements would be so minute that the appearance in them of so subtle an effect as the colors might be expected to have would hardly be likely. In fact, such investigation, with negative results, had already been reported.<sup>28</sup>

Besides, the writer had a feeling that the effect of a bright light, for instance, might consist not so much in an actual stimulation as in a "sustaining" of any activity which might be going on. Such an influence should show best on some form of rather monotonous work which could go on for an appreciable length of time in routine fashion; something comparable to work in a factory, where there are a few well understood stimuli and half-automatic responses, occurring over and over. Rivers<sup>31</sup> and Hollingworth<sup>17</sup> both used typewriting. Typewriting could obviously not be used in the present study; but something of this general nature, a continuous series of reactions to simple stimuli, was the type of task desired.

It finally occurred to the writer that instead of single reactions, and a time measurement of each, a continuous series, with timing only of the whole series, could be arranged; such a test would give exactly the sustained routine activity desired, and would also accommodate itself much more easily to the total test programme. The test, as finally worked out, may be briefly described. For signals two telegraph sounders were used; one, giving a sharp, loud click, to the right of the subject's table; the other, sounding duller and less loud, to his left. On the table, mounted on a small block of wood about 5 by 9 in. in size, were two keys, close enough together to be operated by the first and second fingers of the subject's right hand. The subject was simply to press the right-hand key when the right-hand signal sounded, and to respond with the other key to the left-hand signal. And the apparatus was so arranged that correct response on the part of the subject to a given signal automatically brought about the presentation of the next signal. But if the subject responded incorrectly, pressed the wrong key, no new signal appeared, and the subject had to rectify his mistake before he could proceed.

The test thus consisted of a continuous series of choice reactions, the subject setting his own rate and the apparatus exactly keeping pace with him, presenting new signals as fast as the old were reacted to. The test was run 4 min. with each color, 2 min. at the beginning of a color period and 2 min. at the end (taking the place of the tapping test, after work with the tapping test was completed, in the routine).

Highest possible speed on the part of the subject was not desired; he was told rather to work along steadily, much as he would in running a typewriter, and to emphasize accuracy rather than speed. Time was marked in 30 sec. intervals; both time and correct reactions were recorded by electric markers on a smoked drum. Only two choices were used, in order that learning might be easy and practice effects not great, and mistakes not over numerous.

The important, and original, feature of the test was, of course, the apparatus by which reactions and stimuli were thus interrelated. This interrelation was brought about by means of a special adjustment between contacts with each signal and with each key, and contacts on a revolving wheel. The wheel consisted of two brass cog wheels soldered together; bearing on the right hand wheel were the contact connecting with the right-hand signal and the contact connecting with the key to be pressed when the right-hand signal sounded, and bearing on the left-hand wheel were the contact to the left-hand signal and the contact to the key to be pressed when the left-hand signal sounded. The key contacts were side by side (that is, bearing on cogs which were side by side on the wheel); and underneath, making on the next cogs below, were the two signal contacts, also side by side, that is, making on cogs which were side by side. Certain cogs on each wheel were filed out, however, and these were so chosen that one, and only

TABLE III

NUMBER OF CORRECT CHOICE REACTIONS IN TWO MINUTES. IN TERMS OF % OF AVERAGE NUMBER, PER TWO MINUTES, FOR THE HOUR

<i>Brightness Series</i>												
Sub.	I. (bright)				II.				III. (dim)			
	First av.	2' m.v.	Second av.	2' m.v.	First av.	2' m.v.	Second av.	2' m.v.	First av.	2' m.v.	Second av.	2' m.v.
And.....	100	2.0	102	6.9	100	8.5	104	8.9	95	7.4	98	7.6
Cur.....	98	5.5	102	5.0	107	5.2	105	2.8	91	8.3	98	7.1
Dav.....	103	4.1	99	3.3	100	4.8	99	4.5	101	4.6	96	6.6
Pep.....	101	2.0	101	4.0	101	1.0	100	4.0	99	2.0	98	4.0
Mor.....	99	4.2	99	5.9	98	5.2	102	3.9	99	6.2	102	3.9
Woo.....	97	3.4	101	2.5	100	5.9	100	4.0	98	2.9	103	4.3
Av.....	99	3.5	101	4.6	101	5.1	102	4.7	97	5.2	99	5.6
(Twelve hours each subject.)												

Hue Series												
Sub.	Red				Green				Blue			
	First 2'		Second 2'		First 2'		Second 2'		First 2'		Second 2'	
	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
Allp.....	100	2.9	102	1.7	100	4.6	103	2.0	96	4.7	99	2.1
Br.....	101	2.5	98	1.6	99	2.5	100	2.1	101	3.1	99	3.1
Bul.....	100	4.5	101	2.5	101	3.1	103	3.3	104	5.3	91	6.1
Chac.....	102	3.1	99	2.1	98	3.2	100	3.3	99	2.9	101	1.6
Tul.....	102	3.0	100	3.0	102	2.0	99	3.0	102	2.0	96	3.0
Woo.....	95	4.2	103	2.0	95	6.0	106	3.9	100	3.9	101	4.4
Av.....	100	3.4	100	2.1	99	3.6	102	2.9	100	3.6	98	3.4
(Twelve hours each subject.)												

The m. v. in the average row is the average m. v., not the m. v. of the averages.

In handling per cents values up to .5% were dropped.

one, cog was presented at any given point. The result was that one, and only one, signal contact, and one and only one key contact, were making with the entire wheel at any given time.

Suppose, then, the subject responds (correctly) to a previous signal by pressing the proper key, thus closing the circuit, and suppose the right-hand signal contact is making with a cog on the wheel. The right-hand signal will be sounded. At the same time the current will go through a magnet, jerking forward a ratchet to engage a succeeding cog on the wheel, so that when the subject breaks the circuit, by releasing the key, a spring action against the magnet will jerk the wheel around one cog's distance. The result will be that the cog which was making with the right-hand signal contact will now be making with the right-hand key contact. And only by pressing the right-hand key can the subject again close the circuit, and proceed further with the reaction series.

The essential feature of the device thus consists of this arrangement by which a given contact on the wheel makes successively with a signal and a key contact, so bringing it about that the reaction called for is the only one which can be completed.\* The apparatus, which was mounted on a base about 4 by 12 in. (it could have been built half this size) and stood about 3 in. high, was in a sound-proof box under the experimenter's table. The cogs made in irregular order, so that the subject was given signals sometimes alternately right and left, again two or three in succession left or right, and in various combinations. After some five months of practice one subject began to learn the combinations; the connections were then simply reversed, and so all the combinations changed. Aside from this, there were no evidences of marked practice effects.

The results were first handled simply as total number of correct reactions in the first and the last 2 min.; this was taken as a measure of what might be called "reaction efficiency" under the given conditions. However, since individual differences, and variations from day to day, were marked these figures were converted into per cents of the average for the day, as were the times in the multiplication test.

It should be mentioned that 1 min. practice was given before the hour began; there was nevertheless evidence of practice effects through the hour, especially with certain subjects. And since no convenient way was found for eliminating such constant errors from the results

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\* The apparatus can be made to handle three, four or more choices, simply by using three, four or more cog wheels with their pairs of contacts. It can be used with any type of stimulus which can be electrically operated, and with any type of reaction. The arrangement would, therefore, seem of some general usefulness. The mechanism used by the writer was experimented with up to speeds about twice those of the average subject, and found perfectly accurate at such rates.

As has been pointed out to the writer by Prof. W. F. Dearborn, the test is somewhat similar to Seashore's "Psychergograph;"<sup>22</sup> the apparatus above described is, however, much more practicable. A mechanism very similar to the writer's has recently been described by McComas.<sup>22</sup>

they may be supposed to make the mean variation somewhat larger than it should be.

In the brightness series the group average shows "reaction efficiency" to be greatest under the medium light, least under the dim. As before, Mor. is an exception, and this time also Wool. Mean variations are also slightly greater under the dim light. Differences between first and last 2 min. periods are hardly consistent or definite enough to allow any significance to be attached to them; the two periods are probably best considered together. In the hue series the group average is least with blue; red and green show no important difference. It can hardly be said that in either series the results are of any significance, except in so far as certain subjects in the brightness series give findings in agreement with their previous records on the tapping and arithmetic tests. But this consistency would seem of some possible importance.

6. *The Effects of Color, as given in the Introspective Reports.* As has already been said, for the major part of the experimentation no introspection was called for. Introspection was obtained, however, for the first and last hours of each term, in order to study (1) group tendencies and the amount of individual variations in report, and (2) the effects of habituation to the colors. The tables are too complicated to present; the findings may, however, be briefly summarized.

In this work introspection in terms of the Wundtian tri-dimensional theory was called for.

This was done only after considerable hesitation. But more of a description than a mere indication as to pleasure value was wanted. And if each subject was left to form his own categories, tabular statement and cross-comparison would have been difficult. The subjects were therefore asked, for a time, to report on the colors as pleasant or unpleasant, exciting or depressing, tensing or relaxing. Two degrees of each quality were recognized; a color could be pleasant or it could be very pleasant, or it could be indifferent, neither pleasant nor unpleasant. The work was done with the four-color brightness and the five-color hue series, and was continued four weeks with the first and five with the second (making up on each case one "unit" where each color occurs once in each position through the hour).

The group totals show, for the brightness series, the two bright lights as most pleasant, the dim least. The bright lights are most exacting, tensing; the dim is most depressing, relaxing; indifference centers with the medium light. All of this was to be expected, except perhaps the pleasantness of the very bright light, which was not found unpleasant once in spite of its brightness. In the hue series, green was most pleasant, white next, blue most unpleasant. Red is most exciting and tensing, blue and white are most often called

depressing and relaxing. Judgments of indifference are rarest under red, showing that this hue is usually taken positively, one way or the other.

The variability of a given individual, from one hour to another, was considerable. For instance, And. reports the medium-bright light twice as very pleasant, and twice as wholly indifferent; Cha. finds blue three times exciting, once very exciting, once depressing; Bul. finds white tensing once, relaxing twice, indifferent twice; Lang. finds blue pleasant twice, unpleasant once, indifferent twice.

Individual averages also differ markedly. Bul. finds all the hues and all the brightnesses uniformly pleasant, with green and the brightest light each called very pleasant once. The dim light is for Fin. and Dav. uniformly unpleasant. Cha. finds red very pleasant, Ohl. very unpleasant. Red is for Bul. relaxing, for Dav. very tensing. The results of continued experience with the colors were quite what would be expected. There was a thinning of the feeling, a dropping-away of associative enrichment and toning, a matter-of-course attitude which paid very little attention to the colors one way or another.

Thus one individual, in the first weeks of his experience as subject, reported the bright light as "exhilarating, hard on the eyes at first, but less hard than the dim light," and later in the same period, "pleasant, more awake." The dim light was disagreeable, unpleasant, sleepy, tensing, bothering to the eyes. After three terms with the experiment he speaks of the bright light as "just an ordinary light, a bit pleasant when first turned on, doesn't mind it at all." And at the end of the period he says that there "is nothing special to report, slightly pleasant, that's all." Another subject reports in the first weeks that the bright light is "exciting, livening, like sunlight," the dim is "easy on the eyes, relaxing; doesn't feel like working." At the end of the year introspection on the bright light is simply that "the dirt spots on the paper show up more;" the dim light "tends to put to sleep, a little depressing."

Similarly, in the hue series, at the beginning of the year, a subject reports red "very pleasant, because of its richness," and again, "very pleasant, it is so rich." Blue is "pleasant, reminds one of the sky and clouds," again is "ghastly, unnatural, unpleasant, exciting" (this the following week!). At the end of the year blue is "neutral" and again "neutral in all ways." Red is "perhaps slightly pleasant" and again "slightly pleasant: it came as something of a surprise." Another subject who at first "didn't like the red a bit" and again found it "decidedly unpleasant, tensing, exciting," toward the end of the year called it merely "not quite so pleasant as the others," and "on the pleasant side." Blue was at first "very unpleasant, disagreeable, tiresome;" by June it had become "quite pleasant, rather restful," and again "quite pleasant, rather restful."



It may be said shortly that there is, if the introspective reports may be trusted, a definite decrease in affective reaction to both hue and brightness, with habituation.

The results obtained in the systematic experimentation may, then, be briefly summarized.

(1) Results with a tapping test, a multiplication test, and a continuous reaction test, suggest a decrease in function under dim, and an increase under bright light. With the other tests no effect of brightness was observable.

(2) The objective measurements showed no effect of hue, independent of brightness, upon the functions tested.

(3) Introspective estimates indicated marked variability, and marked individual differences, in the feeling-attitude toward brightness, and especially toward hue. A marked decrease in affective reaction to both hue and brightness, with habituation, is the only feature common to all the introspective reports.

#### IV. SUMMARY AND CONCLUSIONS

So much, then, for the writer's experimentation. It remains to evaluate these findings in the light of previous work, and to summarize what seem to be the most probable conclusions with regard to the problem.

First to be considered (1) are possible differences among the hues, in their effects upon mental and physical activity. As has already been intimated, the writer feels that there is no adequate evidence in previous work to show any such differences. He has found no such differences in the present experimentation. The common notion that certain hues have a marked influence upon emotional tone and mental work is the result, he believes, of such subtle and pervasive influences, arising from figures of speech, custom, artistic and social convention, and everyday association of certain hues to other sensations of a strong affective toning, as have already been mentioned in the historical review. The misconception may also be due in part to a failure to differentiate the influence of brightness from the influence of hue.

If there is an effect of illumination upon mental work, it would appear, then, (2) that brightness must be the important factor. Certain of the experimental results obtained by the writer have suggested a slowing of mental work under dim light and a stimulation under bright light. Such an effect is at least not incompatible with what little previous work has been done in this field; and the hypothesis of a stimulat-

ing or dynamogenic influence of brightness upon mental work and neuro-muscular tone seems by no means unreasonable. It need hardly be added that, if there is such an influence, it is a matter of great general importance, from a practical point of view.

It must, however, be emphasized at once that the writer's positive findings are few and far between, and that their reliability is by no means all that could be desired. The writer has argued that in such a study the interrelation of all the results should be taken into consideration, in judging of the significance of the findings. And he has based his conclusions on (1) the consistency of the results obtained from each subject on each single test, (2) the appreciable agreement among different subjects on the same test, and (3) the consistency of the results from test to test with each subject. But even with such a liberal basis of interpretation, it can hardly be said that the results do more than "suggest" the inferences which have been drawn.

It is also obvious that fifteen minutes of experimentation in a laboratory dark-room tell us little about what might be the effect of the same brightness continued throughout the day, in shop or schoolroom. Fundamentally, the problem is not a laboratory problem, and needs study of a larger sort, in schoolroom or shop. The present study can be thought of as contributing little more than a definition of the problem, and a suggestion of methods. But the writer is convinced that there is here a real problem in applied psychology, well worth more extended study.\*

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\* But not, let it be repeated, in the laboratory! The writer finds himself, after three years of intensive research with mental tests, going over this manuscript with oddly mixed feelings. Not so long ago he returned from a four days' visit to a city of about 11,000 population: during those four days, he personally tested every child of the 1,500 children in that school system. And in contrast, three years of weary, discouraged puttering in a dark room, with a total of twenty-six subjects! He would hardly think of basing any very serious conclusion, now, on less than that many hundred. But by all this he does not mean to imply a loss of interest in the older type of experimental problem. Rather he believes that practically all of the laboratory problems can be handled, in the large, by the "test" method, with such massed cases. He is convinced that recent developments in the field of group-tests are much more important for experimental psychology than is usually realized. The group-test is really, he believes, a new method, of splendid promise, applicable to the entire range of psychological investigation. And it is to the "group-test" that he looks for a final conclusive treatment of the majority of the older experimental problems: that is, a majority of those problems which may be worth such treatment!

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## THE NATURE OF THE AFFECTIVE JUDGMENT IN THE METHOD OF PAIRED COMPARISONS<sup>1</sup>

By M. YOKOYAMA

In a previous article the writer has reported data which show the manner in which judgments of pleasantness are dependent upon the simultaneous operation of the color and the form of geometrical figures cut from colored paper.<sup>2</sup> These results were obtained by the method of paired comparisons, the most usual tool in the experimental study of affection. In this investigation a great many introspective reports were obtained from observers at various stages of practice but were not fully presented for the reason that they threw but little light upon the immediate problem. These protocols, however, bear quite intimately upon the nature of the material which the method of paired comparisons yields, and it is proposed in the present paper to study them with especial regard to the nature of the affective judgment.

Titchener in 1908, discussing the nature of affection in the light of the experimental work available at that time, concluded that affection could be neither sensation nor attribute of sensation.<sup>3</sup> Since then, however, the face of introspective psychology has been changed by the realization of the need for a constant differentiation between mental process on the one hand and meaning on the other.<sup>4</sup> Young noted how affective judgments may be meanings of preference, not dependent directly upon affective process;<sup>5</sup> but he was not concerned with

<sup>1</sup> From the Psychological Laboratory of Clark University. The present paper is abbreviated for publication by the omission of many introspective reports. A bound manuscript containing detailed protocols will be found under the same title in the Clark University Library and may be borrowed under the usual courtesies of library exchange.

<sup>2</sup> M. Yokoyama, Affective tendency as conditioned by color and form, *Amer. J. Psychol.*, 1921, 32, 81-107.

<sup>3</sup> E. B. Titchener, *Psychology of Feeling and Attention*, 1908, 1-168, esp. 33-78, 167 f.

<sup>4</sup> Orth's distinction in 1903 between feelings and *Bewusstseinslagen* was, of course, prior to the full recognition of the relation of meaning to conscious attitude: J. Orth, *Gefühl und Bewusstseinslage*, 1903.

<sup>5</sup> P. T. Young, An experimental study of mixed feelings, *Amer. J. Psychol.*, 1918, 29, 237-271, esp. 252-258: cf. also C. E. Kellogg, Alternation and interference of feelings, *Psychol. Monog.*, 1915, No. 79, esp. p. 89.



raising the fundamental issue, and sought primarily to rule out from his results a class of cases that was of a different order from those with which he was mainly concerned. The present writer seeks to raise the major question whether the method of paired comparisons can ever be relied upon to yield material other than affective meanings. He has no hope of settling finally the issue of the existential nature of affection, but believes that a demonstration of the inapplicability of this orthodox method to an experimental study of affective processes is worth the making. Certainly, so it seems to him, it is no longer possible for the experimental psychologist to assume that the mere utilization of a method assures results that exhibit laws of affective process.<sup>6</sup>

### Observers

Our O's were peculiarly well qualified to bring out distinctions between meaning and process, because they had been sensitized in the 'atmosphere' of the Clark Laboratory to the process-nature of introspective material and to a persistent distinction between *Beschreibung* and *Kundgabe*. On the other hand, the very fact that the problem of meaning is so much 'in the air' at Clark may perhaps be thought to have predisposed them unfairly toward the discovery of meanings. However, without some such sensitization, which amounts to little more than a training in the refinement of modern introspection, it would have been hopeless to seek results which bear upon our problem at all.

An O, in approaching this problem, is apt to establish by his protocols his own theoretical bias. Hence we note here these biases, as indicated in formal statements by the O's themselves. Observer B, the director of the laboratory, stated that for a number of years he had been sceptical of affection as an existential mental process and had been becoming constantly more convinced that it could be adequately accounted for as meaning or act. Observer D, a psychologist of some years standing, stated that she began the experiment without definite theoretical biases, but with more or less sympathy toward the theory that affection may be accounted for as meaning. Observer F, assistant professor of psychology, stated that he had always held that affections were simple and ultimate psychological processes, capable, except in clearness, of attributive description and coordinate with sensations and

<sup>6</sup> As, it would seem, could Titchener, *Philos. Stud.*, 1902, 20, 382-406; S. P. Hayes, *Amer. J. Psychol.*, 1906, 17, 358-393; W. S. Foster and K. Roese, *ibid.*, 1916, 27, 157-170.

images. Observer P, a graduate student of three years training, was less interested in the general theoretical consideration of affection, but stated that he believed in the possibility of rendering reports upon feeling as independent mental elements. The protocols of Observer M are not considered in this paper for the reason that her lack of introspective training at the time of the experiment leaves her reports equivocal with respect to the point at issue. All the O's, it may be said, however, insisted that they approached the experiment with an open mind, and that their sole motive was to make accurate and detailed descriptions and analyses of the experiences observed under these particular experimental conditions.

### *Quantitative Summary of Introspections*

The absolute frequencies of the occurrence of various affective items in the protocols are presented in Table I, which thus constitutes a rough summary of the introspections and of the individual differences of report among O's. In general, it may be said of the table, the first items refer to the two stimuli compared: "2 P" (=two pleasantnesses) means that pleasantness was reported in connection with each stimulus, "P and U" (=pleasantness and unpleasantness) means that one stimulus was pleasant and the other unpleasant; "1 P" (=one pleasantness) means that one stimulus was pleasant and that there was no affection connected with the other stimulus; and so on. The item "affective or aesthetic characterizations" covers reports of the stimuli as "nice," "good," *etc.*

TABLE I

ABSOLUTE FREQUENCIES OF INTROSPECTIVE REPORTS AS CHARACTERIZED BY THE AFFECTIVE CONTENT INDICATED. P = PLEASANTNESS; U = UNPLEASANTNESS

Observer	B	D	F	P
2 P.....	12	4	2	1
P and U, or P and not-P.....	3	0	1	9
2 U.....	2	1	2	1
1 P.....	9	0	3	16
1 U.....	1	3	16	6
Affective or aesthetic characterization.....	6	30	7	5
No P or U.....	1	0	17	3
P or U not connected with stimulus.....	3	1	4	3
Total number of introspections, excluding duplications.....	31	36	34	39

*Summary of Introspections*

*Observer B.* The *Aufgabe* was seldom focal for B in the foreperiod. He was only vaguely aware of the instructions in terms of rigid bodily position and the fixation of attention upon the screen.

The process of comparing varied for him from time to time. As a rule, however, the initial tendency to determine a preference manifested itself with the first perception of one stimulus of the pair and took the form of an incipient absolute judgment of that stimulus; a judgment which never came immediately, but was always preceded by organic and kinaesthetic processes of a certain pattern, accompanying the visual perception and forming in turn the basis of the judgment. B's attention would then shift to the other stimulus and the same procedure would be repeated. During this period he would form a tentative, preferential judgment. The reaction and the announcement of the final judgment usually came with a second perception of one of the alternatives. In cases of difficult judgment the processes were more complex and involved. B would fixate now one and then the other stimulus several times in quick succession, with consciousness dominated by a general emotional stir-up that would seemingly delay the usual organic "building-up-process-for-pleasantness" (see below). In judgment of this kind, the decision was generally accompanied by awareness of the *Aufgabe*.

B's affective judgment was mediate throughout. He never observed pleasantness and unpleasantness as existential mental processes. He reported them as conscious attitudes or "meanings," carried by complexes of organic and kinaesthetic sensations or images. In order to declare a preference, therefore, it was always necessary for him to have this sensory mediator, and he was thus always in search of some such cue. (In the writer's previous paper he has characterized this attitude as a sensorial attitude.<sup>7</sup>)

In the early stages of the experiment, B was uncertain of the proper cue for affection. At first he sought to base his judgments of pleasantness upon an *Eindringlichkeit* which consisted of the bare perception of the stimulus with high degree of clearness, intensity and distinctness (*Schärfe*) plus the kinaesthetic response of fixation and head-movement toward the stimulus.

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<sup>7</sup> *Op. cit.*, p. 88.

"Eye-movement went back to right stimulus and the eye stuck there, as pattern of eye-kinaesthesia, which was quite intense and carried the meaning: 'Striking color.' Then at once automatically throat-kinaesthesia, which meant incipient judgment for the right" (Nov. 18, 1919). "It was *eindringlich*, i.e., a kinaesthetic context of receptivity. This kinaesthetic context was mostly eye-kinaesthesia, but had a little bodily adjustment in it too. . . . I do not know if anything carried the *P* or was the *P*. It may be that part of the eye-kinaesthesia in the *Eindringlichkeit*-context was or meant *P*, but I am not sure" (Nov. 26, 1919).

In more advanced stages, the criterion of pleasantness changed from *Eindringlichkeit* to what he termed a "building-up-process-for-pleasantness," which consisted of a suffused organic "glow," in quality something like the organic ache of stretching a fatigued muscle:

"Fixation of yellow with yellow both distinct and clear, and an organic attitude (organic sensations in trunk welling up, like a very mild 'affection' in the popular meaning of that term) that I might feel for a pretty tone or a little child. In content simply a pressure-pain glow (pleasant pain, of course) in the stomachic region or just above. Not unlike appetite in sensory nature; totally different in meaning. . . . When the glow got to a given pitch, it meant that the judgment was formed" (Dec. 11, 1919). "I am sure that the thing that builds up *P* is something that is sensory in nature and belongs to the tactual-kinaesthetic-organic groups of qualities. It is very weak and never very clear. I can scarcely localize it, yet I am sure it is in 'me' and not in my legs and arms. I should say it was above the abdomen and partly in the trunk, but that it also involves the facial muscles. Verbally I should label it a *glow*, perhaps a warm glow; but I do not think the sensation warmth actually figures. The phrase is intended to convey the meaning rather than the content. As sensory content I take the thing to belong to the class of pleasant organic pressure-aches, in which the pleasant ache of stretching fatigued muscles belongs. But it is nowise so distinct as the stretching complex; it is just a tiny, little, obscure, glowing tingle, as much pained as a bit of food going into the stomach under one of Carlson's appetites, and by no means so definitely localized" (Dec. 20, 1919).

Although he found exact localization impossible (*vide supra*), at times he reported this organic complex in chest and face, and at other times in the region of stomach and trunk. The 'building-up' process was slow at the very first; later it became very rapid, and still later its contents began to drop out of consciousness. In the final stage, it degenerated to a mere kinaesthesia of adjustment toward the stimulus wherein it was differentiated from '*Eindringlichkeit*' only by presence of a limited amount of "glow" or a trace of the kinaesthesia of a smile. It was at this stage of the experiment that B remarked that mental content might not be essential to the making of a preferential judgment.

"The attitude of preference developed for the right. It is a bare skeleton of the old 'building-up process' now. I look at the stimulus and turn (in imagery, I suppose) toward it, and sustain this attitude. The whole thing came in this time not immediately, but it built up very rapidly. When it had got built up, it was absolutely nothing more than a kinaesthesia of adjustment toward the stimulus, and it meant: 'I prefer that'" (Jan. 20, 1920). "There is a bit of process to the *P*, but less than there used to be; i.e., in perceiving the circle there was a hint of a glow; conscious attitude, perhaps a trace of the kinaesthesia of a smile. Most of the *P*, however, was sheer meaning; i.e., the circle was a pleasant circle, I knew it for a pleasant circle. My guess is that I am reacting to the meaning of *P*, that the content that carries the *P* is not essential to it, but generally exists" (Jan. 27, 1920).

Unpleasant experiences were infrequent with B and he furnished no complete description of a carrier of unpleasantness. One of his reports states:

"I can't describe *U*. I think it is some sort of organic pattern in my body. I actually believe that it is as much like the dull pressure-aches of nausea as *P* is like the bright contacts of tickle; but it is a far cry from *P* and *U* to these simple sensory things" (Dec. 20, 1919).

*Observer D.* Throughout the foreperiod, D maintained a relaxed bodily position. She had a vague consciousness of the *Aufgabe* to judge and to introspect carried in terms of kinaesthesia in throat and neck and relatively intense strains of the eyes from fixation upon the exposure apparatus.

Her consciousness during the main period consisted chiefly of visual and kinaesthetic-organic experiences. The initial act of preferring for her usually took the form of an attitude of incipient acceptance or rejection of one or the other stimulus, accompanied on many occasions by verbal (imaginal or actually innervated) characterizations of the stimuli. In all other respects her procedure in comparing was similar to B's.

D's attitude of acceptance consisted mainly of kinaesthesia in the head (an incipient nod) and an organic "glow" (chiefly circulatory and respiratory), or of general bodily relaxations and such vocimotor processes as: "That's a nice color;" "rather nice;" "it's a nice shape;" etc.

"When the red was fixated, there was a pleasant glow, accompanied by imagery in vocimotor terms: 'That's a lovely red.' I am retrospectively aware that there was a brief arrest of respiration, followed by a deep inspiration which carried the meaning of 'general satisfaction'" (Nov. 13, 1919). "My eyes followed the outline of the figure counterclockwise from lower right-hand corner. At the same time there was, besides the eye-movement-kinaesthesia, a slightly changed bodily attitude, especially involving kinaesthesia from holding head in particular way. It meant: 'That's a nice compact figure'" (Nov. 18, 1919). "There followed a particular kinaesthetic and organic attitude that meant my affective reaction to this particular stimulus: 'Rather nice'



is what this bodily attitude meant. . . . The consciousness that conveyed the preference was a very vague affair, partly respiratory (as slight inhibition of breathing), partly kinaesthesia of the head (an incipient nod), partly the very turning of the eyes to the accepted stimulus" (Dec. 9, 1919). "There was some eye-kinaesthesia as my eyes followed the outline of the figure, and there was an 'evaluating consciousness' which consisted chiefly of mild but 'pleasantly toned' organic sensations (chiefly respiratory and circulatory) and a bit of throat-kinaesthesia that carried the meaning: 'It's a nice shape'" (March 9, 1920).

The attitude of rejection was not fully described. It seems to have been essentially a complex of kinaesthesia in the region of the diaphragm (constrained breathing, a feeling of tightness), throat-kinaesthesia (similar in quality to nausea), and the accompanying verbomotor processes meaning: "That's not so nice;" "dislike;" etc.

"Next came a kinaesthetic set which carried the meaning: 'That's not so nice a figure; it takes too much eye-movement to see it clearly'" (Nov. 18, 1919). "Then followed an attitude of incipient rejection of this stimulus in terms of kinaesthesia in diaphragm region, i.e., constrained breathing; and a bit of throat-kinaesthesia that was related to nausea" (Jan. 8, 1920).

Like B, D found no affective elements as distinguished from sensations. *P* and *U* to her were simply conceptual terms, embracing, among other things, affective characterizations such as "nice," "lovely," "not nice," etc., and designating at the same time an attitude of acceptance or of rejection, which was in content nothing more than a group of kinaesthetic and organic processes. These organic and kinaesthetic sensations appear to be for her a *sine qua non* of affectively toned experiences, for the reason that *P* and *U* are regarded as meanings carried by these organic and kinaesthetic sensations.

*Observer F.* The foreperiod in the early part of the experiment may be characterized for this *O* as a period of active attention. His eyes were always fixed upon that portion of the screen where the left-hand stimulus would appear. There was a focal visual perception of a limited part of the screen, and intense kinaesthesia in brows, eyes, jaw, neck, chest and abdomen; all of which constituted *F*'s attitudinal response to the *Aufgabe*. Later on this attitude gradually changed into one of passive attention, in which the kinaesthetic and organic processes were very much less marked than before and the visual perception of the screen was alone focal.

In the initial stages of practice, the main period was especially rich in affective and kinaesthetic experience, although the clearest part of consciousness was always the visual perception of the stimuli. In these stages *F*, like *B*, made a tentative, absolute judgment of *P* and *U* separately upon each

of the alternatives before determining his preference. These judgments were almost invariably accompanied either by some kinaesthetic-organic adjustment (a nod, strains in the eyes, kinaesthesia in nose and upper lip, or the like) or by certain imaginal or actually innervated verbal expressions, such as: "not so good;" "blue, fine;" *etc.*

"When the screen was raised I was aware of a rapid focal visual perception of the right-hand form. Great *P* raising rapidly to an intense maximum. Verbomotor processes: 'Oh, that one' accompanied by motor image of drawing my hands apart; left hand upward and right hand downward" (Nov. 13, 1919). "Very focal and clear perception of it (stimulus) with rapidly appearing *P*, which rapidly increased in intensity. Verbomotor 'Blue, fine.' Rapid relaxation of bodily strains, deep inhalation and closing of the eyes" (Nov. 13, 1919). "Non-focal kinaesthetic sensations (actually innervated) in nose and upper lip (facial expression of disgust). This meant to me a very unpleasant stimulus" (Nov. 18, 1919).

The termination of preferential judgment usually came while *F*'s eyes were firmly fixing the stimulus and was followed by general bodily relaxations. The difficult judgments were, as for the other *O*'s, characterized by several rapid shifts of fixation and by the presence of unpleasant feeling and strains.

In the later stages the process of comparing became highly mechanized and the final judgment came immediately upon perceiving both stimuli, without even slight *P* or *U* entering consciousness. *F*'s judgment now was reduced to a motor attitude, which consisted of the visual fixation of the stimulus preferred and of his mechanical reaction upon the key.

"This visual perception was terminated by closure of the eyes which was either accompanied by or immediately followed by a very mechanical reaction, both of which were without other antecedent so far as I am aware. . . . I had no affective processes in this experiment whatever" (Jan. 29, 1920). "This visual perception was terminated by a series of processes which took place in the following order. (1) Absolutely mechanical and automatic reaction on the key without other conscious antecedent. (2) Closure of the eyes. (3) Rapid exhalation. (4) Relaxation of the widespread bodily and muscular strains of the foreperiod. . . . I am sure there were no affective processes in the entire experience" (Feb. 12, 1920). "Almost at once upon the beginning of the visual perception I found that I was reacting on the key in a perfectly mechanical fashion and without other antecedent" (March 4, 1920).

*F* reported *P* and *U* as if they were existential mental stuff. They were always accompanied by organic or kinaesthetic processes of some sort, but *F* never saw any connection between these processes and the *P* and *U*. He habitually noted the intensity and the temporal course of a given affection: "Very focal and clear perception of it [stimulus] with rapidly

appearing *P* which rapidly increased in intensity;" "relaxation of general bodily strains and rapid decrease in the intensity of *U*." On the other hand there is nothing in *F*'s reports that insists unequivocally upon the independent status of affection; most of his statements could have been made of conscious attitudes. His judgments, moreover, appear to have been conditioned directly upon the stimuli, especially in the final stages of the experiment, and not to have been consciously mediated by the reported *P* and *U*. (Cf. quotations above.)

*Observer P.* *P*'s mechanism of comparing was similar to the other *O*'s. The foreperiod was characterized for him by the 'attitudinal set' or '*Einstellung*' for carrying out the instructions. This set consisted of muscular strains about neck and shoulders, and organic sensations about lower oesophageal and upper stomachic regions. A general tenseness of body remained constant, but non-focal, until the appearance of the stimuli.

With exposure *P* fixed his attention alternately upon the two stimuli, evaluating each of them in its turn. Rapid, telescoped, verbal characterizations at times would accompany the perceptions: "That red's good, but orange not bad;" "triangle," etc. At the beginning of the evaluating process the non-focal tenseness of the foreperiod would increase momentarily in intensity, and then gradually break down. There would then be a general 'welling up' of organic sensations or an organic 'glow' in the region of stomach and oesophagus, accompanied by *P* or *U* as the case might be, and issuing in the formation of the preferential judgment. When judgment was difficult, the period of evaluation would be much prolonged and several shifts of fixation would take place. In these cases consciousness was dominated by unpleasantly toned organic processes, and the decision occurred only after reappearance of the awareness of the *Aufgabe*.

"This [perception of the form at right] was followed by rapid welling up of acute, intense, organic sensations, localized in oesophagus and stomach, rather intense *U*. Increase in the bodily strains, especially eye-kinaesthesia and in skull" (Nov. 15, 1919). "Focal visual perception of the stimulus at the right and very non-focal visual perception of the stimulus at the left and very non-focal, indistinct, vocimotor-auditory imagery of 'red,' accompanied by slight *P*. . . . Then stimulus at right dropped out to a very non-focal level. The stimulus at left then again became focal. This was accompanied by the slight organic sensations localized in the region of stomach. Then reaction to the 'red-consciousness'" (Dec. 23, 1919). "Fairly focal visual perception of the stimulus, accompanied immediately after by the image (visual) of the left; then *U*. This was accompanied by organic sensations around stomach and oesophagus, a slight increase

TABLE II  
OCCURRENCE OF VARIOUS DATA OF CONSCIOUSNESS (MEANINGS, PROCESSES) AT VARIOUS STAGES OF THE EXPERIMENTAL  
PROCEDURE. 4 OBSERVERS. P = PLEASANTNESS; U = UNPLEASANTNESS

Obs.	Datum	Initial Stage	Second Stage	Third Stage	Final Stage
B	P and U as: Process-basis of P and U.  Genetic decay of process-basis.	Psychological meaning Eindringlichkeit = P. Organic and kinaes- thetic complexes = P. Rich and well-marked.	Psychological meaning Building - up - process (organic) = P. Or- ganic and kinaes- thetic complexes = P. Rich and well-marked.	Degenerated psycho- logical meaning A trace, merely, of building - up - pro- cess (organic) = P.  Partial decay of or- ganic process. Rarely bodily atti- tude only.	Practically biological meaning P = stimulus itself. Rarely incipient tend- ency for building- up-process.  Almost complete de- cay of organic pro- cesses. Usually bod- ily attitude only.
D	P and U as: Process-basis of P and U.  Genetic decay of process-basis.	Psychological meaning Attitude of acceptance (thetic) = P. Attitude of rejection (thetic) = U. Rich and well-marked.	Psychological meaning Attitude of acceptance (organic and kinaes- thetic) = P. Attitude of rejection (organic and kinaes- thetic) = U. Rich and well-marked.	Practically biological meaning Vestigial remains of attitudes of acceptance and rejection.	Practically biological meaning Partial but incomplete decay; occasional or- ganic and kinaesthetic processes.
F	P and U as: Concomitant re- levant sensory processes. Genetic decay of relevant pro- cesses.	Existential mental processes, either unique elements or conscious attitudes Organic and kinaesthetic processes.  Rich and well-marked.	Existential mental processes, either unique elements or conscious attitudes Organic and kinaesthetic processes.  Rich and well-marked.	Preference = biological meaning No longer P and U as mental process Bodily attitude of fixation of stimulus, but no prominent organic or kinaesthetic pro- cesses. Almost complete decay of affective and organic processes.	Preference = biological meaning No longer P and U as mental process Bodily attitude of fixation of stimulus, but no prominent organic or kinaesthetic pro- cesses. Almost complete decay of affective and organic processes.
P	P and U as: Concomitant re- levant sensory processes. Genetic decay of relevant pro- cesses.	Existential mental processes, either unique elements or conscious attitudes Organic and kinaesthetic processes.  Rich and well-marked; practically no decay.	Existential mental processes, either unique elements or conscious attitudes Organic and kinaesthetic processes.  Rich and well-marked; practically no decay.	Existential mental processes, either unique elements or conscious attitudes Organic and kinaesthetic processes.  Rich and well-marked; practically no decay.	Existential mental processes, either unique elements or conscious attitudes Organic and kinaesthetic processes.  Rich and well-marked; practically no decay.



in muscular tension (general, non-localizable) and by kinaesthesia about eyebrows" (Jan. 21, 1920). "Rapid welling up of intense organics about the stomach and oesophagus, accompanied by *P*" (Jan. 31, 1920). [Difficult case]: "Non-focal rapid vocimotor imagery: 'Huh, neither' followed by very rapid shift of fixation from right to left and back again (4 or 5 times), accompanied by organic sensations. Then slight awareness of *Aufgabe* and vocimotor imagery: 'Oh, left is a little better'" (Nov. 18, 1919).

In the advanced stages *P*'s judgment was mechanized and was made at times without presence of reportable *P* or *U*.

Like *F*, *P* reported *P* and *U* as if they were mental processes distinct from sensations. He noted their intensities and durations in making comparisons. The organic and kinaesthetic sensations, which were concomitant with affections, he almost always localized in the region of the oesophagus and of the stomach. He gave no clear-cut descriptions of these sensory processes. It seems that the organic glow, usually concurrent with *P*, was of a milder, more diffused nature and thus differentiated from the acute, intense, straining quality that appeared closely associated with *U*.

#### General Summary

Table II constitutes a summary of the protocols fractionated into successive genetic stages for those *O*'s for whom marked serial changes of consciousness appeared. The table gives for *B* and *D* the nature of the meanings of *P* and *U*, the process-bases of these meanings, and a characterization of the extent of the decay of the sensory content. In accordance with Titchener's context-theory of meaning,<sup>8</sup> the table uses the term "psychological meaning" for a meaning that is reportable both as such and in terms of its sensory carrier, and "biological meaning" for a meaning reportable as such that occurs with little or no observable sensory basis. *F* and *P*, who imply the independent elementary status of feeling, are summarized in the table by a statement of concomitant organic processes and a characterization of the decay of the total conscious affective experience.

The question of the mental status of the affective process comes up as an issue between observers. *B* and *D* were emphatic in their denial of *P* and *U* as directly observable, nonsensory, unique, mental processes, but reported them consistently as unique meanings, carried by complexes of organic

<sup>8</sup> For general statements of the context-theory of meaning, see E. B. Titchener, *Thought-Processes*, 1909, 175 ff.; *Text-book of Psychology*, 1910, 367 ff.; H. P. Weld, *Meaning and processes as distinguished by the reaction method*, *Titchener Studies in Psychology*, 1917, 181 ff.



and kinaesthetic processes. In any case observable processes were to be expected, since meanings in the early stages of their life history can be expressed in terms of existential sensory process. The issue hinges therefore upon the determination of the sphere in which *P* and *U* are unique and possess independent status, the location of this sphere in the realm of observable mental process. Unfortunately the question is one that we cannot finally determine here; we must content ourselves merely with an additional contribution. B called *P* an "attitude-for-pleasantness" and D an "attitude of acceptance." The organic and kinaesthetic bases of these attitudes were (especially for B) very vivid in the early consciousnesses; they underwent, however, the usual degeneration to which the sensory carriers of meaning are subject, and as the experiment progressed they became finally illusive tags of sensation or disappeared altogether. In other words, "psychological meaning" became "biological." In contrast to B and D, F and P reported *P* and *U* as if they were existential mental processes, although they found no occasion especially to assert their existentiality. It is not to be expected that an *O* will especially assert the obvious, and it is thus a reasonable interpretation of their reports to infer that the process-status of feeling was obvious to them and that a present feeling required therefore nothing further than its mention. Nevertheless, when the reports of F and P are taken in the light of the scepticism of B and D, we may perhaps wonder whether the readiness with which *P* and *U* are spoken of as such is a readiness that comes from their independent existential status or the readiness with which conscious attitudes are reportable as mental units. The latter interpretation is supported by the course of F's consciousness (Table II), in which a degeneration of the affective experience was covariant with the degeneration of the concomitant organic and kinaesthetic sensory content.

Taken all in all, our findings may be summarized with respect to the particular point at issue as follows:

1. *P* and *U* may be meanings for any observer; for B and D always; for F consistently at one stage; for P occasionally.
2. Evidence for *P* and *U* as nonsensory existential processes is weaker than evidence for them as sensory mental processes. B and D explicitly describe them as sensory; F and P do not explicitly describe them but merely name them. There is no statement in any introspection that could not have been made of a conscious attitude.

3. Organic sensory content is the *sine qua non* of *P* and *U*, except in advanced stages of degeneration; for *B* and *D* as carriers of the pleasant and unpleasant meanings; for *F* as concomitant or carrier; for *P* as concomitant.

4. Conscious *P* and *U* drop away from the affective perception in the course of its decay, thus following the law of perceptual contextual sensations.

5. The loss of *P* and *U* is concomitant with the diminution or disappearance of the sensory organic contents of consciousness.

6. The two preceding items of fact indicate that *P* and *U*, whether as sensory or nonsensory, are to be thought of as integral to the total organic complex.

It follows therefore:

(a) That *P* and *U* (*of the method of paired comparisons*) are most universally and definitely statable as meanings;

(b) that on the side of process *P* and *U* are predominantly sensory; and

(c) that there exists a portion of the process-aspect of *P* and *U* which is equivocal in its essential nature (*i.e.*, it is not certain whether it is sensory or nonsensory), but that this equivocal portion of the process-complex follows a law of sensory decay.

The case is by no means conclusive against the status of affection as independent mental process. For one thing it applies only to the method of paired comparisons. It does show, however, that much greater concern needs to be given to the part played by meaning and the illusive carriers of meaning in the consideration of feeling as elicited under this method. Upon the broader extension of these conclusions to the problem of feeling in general the writer is unprepared even to make a surmise. He feels, however, that his results indicate that the method of paired comparisons can no longer be taken as a typical laboratory setting for affections of process-nature, and that the experimental establishment of the process-affection is therefore more than ever in need of an experimental method.

# A STUDY IN LOGICAL MEMORY<sup>1</sup>

By SARAH D. MACKAY AUSTIN

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## INTRODUCTION

(1) *The Problem.* The chief aim of this investigation has been to see if certain of the laws established for the recall of nonsense material will hold for sense or logical material as well. The special phase of this problem which has been studied is to determine whether divided repetitions prove more effective than accumulated repetitions in learning material with meaning. The more important subordinate problems which have developed from this larger problem are: the relation of logical learning to rote learning, or memory for ideas as opposed to words; and, in particular, the effect for recall of distributed as compared with accumulated repetitions; and the influence of time (the curve of forgetting) upon the amount and character of the material retained. Although the chief interest in the problem has lain in the quantitative aspect of the process of remembering, careful introspections taken

<sup>1</sup> From the Psychological Laboratory of the University of Michigan.

throughout the experiments show something of the qualitative nature of the problem as well.

(2) *Literature.* Relatively little of the vast amount of literature which has been published on memory and the learning processes has a direct bearing on this problem. Only that work which deals more or less directly with the learning of logical material will be briefly reviewed.

(1) *Learning of (a) Nonsense Material and (b) Logical Material.*

(a) The pioneer experiments of Ebbinghaus (3) and Jost (7) in the field of memory were done almost entirely upon material without meaning. Following them have been investigators who have used both material without and material with meaning, as Radossawljewitsch, who used both in his study of forgetting. A number of experiments have been made upon logical material, but usually for verbal recall, not for the memory of ideas as opposed to words. Balaban (1) studied the difference between logical and mechanical memory, using syllables, words, and compound words.

(b) Henderson (6) has carried out probably the most complete research with logical material. He had 212 subjects, ranging from school children of the fifth grade to graduate students in college. A type-written copy of a short prose selection was handed to each subject to be learned in 3 min. At the expiration of that time the subject was required to write as exact a reproduction as possible. The aim of the experiment was to determine the amount and character of the material retained. The subject was then tested two days and four weeks later.

The results of 259 complete papers were scored on the basis of the story, topics, sub-topics, details, and words. All words of the original text that were reproduced in their former contexts were scored full value. Commonplace words, articles, prepositions, and conjunctions were not scored when reproduced out of their context. On the other hand, an unusual word was regarded as remembered even though it appeared in the wrong context. Henderson found that the older students got ideas more than words, and that there was a slight tendency for more advanced students to remember better. He found also that a single reading, concentrated, was nearly as effective for immediate reproduction as 3 min. of study. Details disappeared owing to generalization and there was a tendency to substitute. The details least consonant with the thought were usually forgotten.

Whipple (18, pp. 394-403) refers to the same process of generalization. Referring to Binet and Henri, he says that they found a tendency to express ideas in their own terms, rather than those employed in the original passage. They estimate that memory for connected sentences is approximately 25 times as good as memory for discrete terms. The general conclusion, we may say, of the investigators using logical material is that ideas are retained, not the original words and that there is a strong tendency to generalize when learning subject-matter with meaning.

(2) *Distributed as opposed to Cumulative Repetitions.*

(a) For nonsense material, Ebbinghaus showed the advantage of distributed repetitions in learning. Using the "Saving Method," he

found a saving of more than 30 repetitions in relearning by distributing his repetitions over 3 days. Jost,(7) likewise, found a saving in relearning with repetitions scattered. He used the "Saving" and "Treffer" methods, and found that 30 repetitions at once tested on the following day were very ineffective as compared with the 30 repetitions distributed within limits. He found an advantage in scattered repetitions in both the methods which he used; a saving in relearning, and also a greater number of successes. He concluded that the "Treffer" method was the better of the two.

Perkins'(15) experiments upon nonsense syllables showed also the greater advantage of repetitions distributed within limits. Her main problems were, first, "to discover if there was a limit to the degree of distribution that might be advantageous" and, secondly, to determine, in cases where more than one repetition was made in a single day, "if learning was easier when more than one day was allowed to intervene between the groups of readings, and to discover, if possible, the most favorable period." The results showed that the highest percentage of success was obtained by the use of one reading every third day. She found that, in general, the smaller the number of repetitions used at a sitting, the higher was the percentage of success.

(b) For sense material. The most recent experiments in which scattered repetitions are compared with cumulative, and which are very closely related to this problem, are those of Edwards(4). He compared the relative value of 6.5 min. as opposed to 4 plus 2.5 min. when studying a page of history. It was so arranged that the total amount of time was the same for all. Half of each class was a review group, half a non-review group. His subjects were high school and grammar grade pupils. The non-review groups studied 6.5 min. and wrote 12 min. and took an examination from 10 to 12 days later. The review groups studied 4 min., wrote for 12 min., and about 5 or 6 days later reviewed for 2.5 min. It was found that the review groups were better without exception in reproduction. The review groups could do sometimes 50 per cent more than the non-review groups per pupil.

(3) *The Curve of Forgetting of (a) Nonsense Material, (b) Logical Material.*

(a) In any memory problem, one phase is its relation to the "Curve of Forgetting." Although material with meaning is retained better than nonsense syllables, the same general tendencies are shown in the rate of forgetting. Working with nonsense syllables, Ebbinghaus(3) found that 42% of forgetting took place after 20 min.; 56% after 60 min.; 64% after 9 hrs.; 66% after 24 hrs.; 75% after 6 days; and 79% after 31 days. That is, forgetting took place rapidly at first, and then more slowly.

Radossawljewitsch(17) also found approximately the same results for nonsense material, *i.e.*, a decided loss at first, then a more gradual loss of the syllables that had been learned. His experiments differed from those of Ebbinghaus only in a smaller amount forgotten at first. He found a relatively slow period of forgetting between 6 and 21 days.

(b) Although the major portion of his work was done with nonsense syllables, note may be made of one experiment by Ebbinghaus on material with meaning. Even after 22 years, he found a retention of stanzas of poetry learned so that they could be said once, perfectly,



and not seen in the interim. Radossawljewitsch(17) also used material with meaning; a portion of Schiller's *Siege of Troy*. Two stanzas were assigned to his subjects who read them through repeatedly until they could reproduce them twice without error. He found that one-third of the material with meaning was gone in 2 days, one-half in 7 days, while material without meaning was lost much more quickly. With both nonsense and sense material, with these two investigators, the same general "Curve of Forgetting" holds, namely that at first forgetting takes place rapidly and then more gradually.

In connection with the curve of forgetting, Bean(2), in his monograph, presents results from his experiments on forgetting of relatively simple motor acts, sensations with relatively little meaning, and sensori-motor experiences. He says that his own results do not warrant altering the general character of the Ebbinghaus curve of forgetting, nor introducing any other general types of curve. But the rate of forgetting is variable, depending on (1) the degree to which the material had been learned in the first place, (2) the distribution of the repetitions, (3) the kind of material learned, (4) the method by which it is measured, and (5) individual difference in retentiveness.

## II. THE METHOD OF EXPERIMENT

(1) *Subjects.* The following persons served as subjects, Prof. W. B. Pillsbury, Miss Nellie Perkins, Mr. J. Winter, and Mr. H. W. Crane, who performed memory tests during one college year for this experiment. Five other persons served as subjects for a few tests, so few that they had to be discarded. The class in Introductory Psychology furnished material for several tests. The writer served as a subject for a year and a half, and then after an interval of three and a half years continued the experiments for about six months. In the following experiments, these subjects will be designated as follows: Prof. W. B. Pillsbury, Subject I; Mr. Crane, Subject II; Mr. Winter, Subject III; Miss Perkins, Subject IV; Miss MacKay, Subject V.

(2) *Materials.* The so-called logical material was of two kinds for each subject. In each case, the subject chose or was assigned material with which he was relatively unfamiliar but in which he was interested to some extent. Subject I used Organic Chemistry and Russell's *Principles of Mathematics*. Subject II used Embryology (Piersol) and History (a detailed account of the Battle of Gettysburg). Subject III used the same Embryology, and Descriptive Geology. Subject IV used the same Embryology, and a History of South America. Subject V used Economics and a History of Scotland. Embryology was a new subject to those who used it in this experiment, and it presented difficulties in terminology as well as in meaning. The assignments in this were very short.

During the first part of the experiment, Subject V used a text book in *Money and Banking*. Experiments begun after an interval of three and a half years were made with the same text-book and the publications of the National City Bank in addition. The history used in these later experiments was the literature sent by Sir Gilbert Parker to the members of the American Bar Association, Wilson's *Division and Reunion*, Walling's *History of Socialism*, and a *History of French Literature*. In every case, the subjects used books they had never seen before.

Previous experiments in the use of sense material have been limited for the most part to sentences, short paragraphs, or two or three stanzas of poetry. In this experiment, the portions to be learned were much longer, and were always of about the same length. In mapping out the material for the other subjects, as well as for myself, I attempted to have the assignments as nearly equal in length as possible. I laid more emphasis, however, upon the logical place to end the assignment than upon having a uniform number of ideas. The assignments varied, with the exception of those in Embryology, from two pages to four or five. It may be noted, in this connection, that the length of the assignment had little effect upon comparative results.

An assignment covering several pages gives a test of logical memory in its practical aspect, since in school, in college, and in practical life, one is never limited to a mere stanza or paragraph. Such subjects as history and economics, for example, were chosen because they were the sort of subject-matter one generally reads, in addition to being logical material.

When I began working on this problem, after a three and one half year interim, it was necessary for me to use shorter assignments than those which have been described, assignments requiring perhaps 5 to 7 min. to read. This method enabled me to have many tests under way at the same time on different assignments of both history and economics.

Note may be made of one great difficulty which arose and made the number of experiments fewer than they otherwise would have been. The subject-matter for the tests was the consecutive material in a chosen text. Thus two pages, let us say, were chosen for the first assignment, the following two pages for the next assignment, the following two pages for the third, and so forth. If history, for example, had been read five times in one day and was to be tested in two weeks, no further experiments could well be performed in that subject until that test had been made. If a second experiment

had been begun on the following pages before the first had been finished, such confusion in both learning and recall would have resulted that both final results would have been invalidated. It would have been impossible to distinguish one assignment from the other when the time for testing came.

The other subject-matter, embryology, could be introduced in the interim, and experiments made in that, but even then there was a loss of time in the experiments extending over longer intervals. As is evident, this difficulty did not present itself in the short-time tests. Repetitions could be made and the subject-matter tested on the following day; and the questions could be answered two days later and the next test begun. I obviated this difficulty, of the loss of time, in my own later experiments by learning various assignments in several books in both economics and history and having several tests overlap.

(3) *Method of Learning.* The total number of repetitions used throughout the experiment was five, which number was chosen arbitrarily. One repetition consisted in reading the assignment through once very carefully. The subject read his assignments at the same time of day, in so far as was possible. He chose his own method, which was to be uniform throughout the experiment; he could read aloud or not as he wished, slowly or rapidly. After reading the assignment, the subject attempted to drop it completely out of mind. This was very difficult to do at first; since the subject knew it should be remembered, it would be recalled again and again in the effort to have all the subject-matter of the assignment available when the test was to be made. Had this method been continued, it would have introduced a great source of error; some portions might have been reviewed more than others, portions incorrectly recalled might have been gone over until they seemed a part of the context, and there might have been a constant effort to keep the material fresh in mind, particularly if the test were two weeks away. A little practice, however, enabled the subjects to dismiss the assignments completely after reading.

The subject read his assignment as directed; for example, five times in one day, once each day for five days, or three times in one day and two times the next, and wrote all he could recall without any assistance of any kind at the expiration of the stated time. Two days later (this time was also chosen arbitrarily) he answered questions which had been carefully made out, covering as nearly as possible all the ideas in the given assignment. The repetitions were scattered over varying intervals of time, from five repetitions in one day, to

one repetition every fifth day. The extreme time-limits for testing were from two hours to three, four, and six weeks, in one case, each. The majority of the tests, however, lay within the limits (including) two hours and one month. Anything interesting in the way of introspection at the time of reading or of testing was noted by the subject.

(4) *Scoring of Material.* The purpose of this investigation, as has been stated, has been to see how many ideas of any given prose assignment could be retained after certain periods of time, after five careful readings at varying intervals. The original assignment was scored very carefully. I read over the text and counted the number of ideas in this way. One half of a sheet of paper was used to score the ideas in the text, and the other half was used for the test. Usually the first and last words, with abbreviations between, of what I called an idea were written down. To give a concrete example from one of the assignments: "Only that statesman, writer, or sociologist, has the hearing of the public to-day who can bind his proposed reforms into some large far-seeing plan." This was abbreviated as follows:

Socialism

Pages 2 and 3

Text	Test
1. Only states, b. f. plan.	2.
2.	2.
3.	3.

In this way, when it came to scoring a test on the other half of the sheet, the original idea was perfectly obvious. The original assignment, in addition, was always at hand for reference in scoring the tests; for often the idea of the subject was correct, although phrased entirely differently. Just as the original text was scored, so the report of the subject was carefully gone over, the total number of ideas in each found, and the percentage of the ideas recalled in each case was calculated. No scoring was done for words or different parts of speech, the purpose being solely to find the percentage of the whole number of ideas in the original text which could be reproduced.

The scoring was done very closely, making the percentages low, particularly in tests extending over several weeks or more. This was necessary because a high degree of accuracy was obtained in an immediate test, say two or twenty-four hours, and many details were given which would have been forgotten in a longer interval.



The first question which naturally arises in regard to the method of scoring is: how were wrong or partial ideas scored? The answer is that, as a rule, either the subject-matter had been entirely forgotten or was recalled correctly. This was found to be true in general throughout the experiment, although there were a few exceptions. Wrong ideas, dates, or figures would be found occasionally, but comparatively seldom. These were simply disregarded if the idea was correctly recalled. If several partial ideas were found in one test, credit was given for a whole idea, but little scoring of partial ideas was necessary.

All figures in the following tables show the percentage of the material retained, that is, the number of correct ideas the subject retained. The first figure shows the original test, the second, the test made with the aid of the question. Thus, in subsequent discussion, 0—28, means that nothing was remembered by free recall, and 28% of the total number of ideas in the given assignment, with the aid of the questions.

### III. TABULATION OF DATA AND INTROSPECTIONS OF SUBJECTS

In the following pages the results of each subject will be summarized, and the introspections of interest noted, with particular reference to their bearing upon the problem of imagery or imageless thought. This experiment has shown that, while comparisons of the results of the same individual can be made, none can be made of the results of different individuals.

Table I, Subject I. (1) *Organic Chemistry*. This material required 15 or 20 min. each day to read. It proved interesting, and no pertinent introspections accompanied it. The greater value of divided repetitions is shown as compared with cumulative.

(2) *Mathematics*. These sections were long and difficult. The introspections show: "Not all understood and did not reread anywhere; general impression vague," on the first day. Second day: some mind-wandering. Third day: still rather blind; last part still hazy. Fourth day: "Felt disinclination for the work, one page not appreciated at all in the reading." Again, the introspections taken at the time of reading another assignment: "Read once, room quiet and conditions favorable, pretty well followed. Conditions favorable on following day, and seemed to understand and often to anticipate."

By reference to the table it will be seen that the assignment read two times the first day and three times the second and tested in two weeks gave 0-28 per cent. The notes taken at the time of reading show that it was not understood at the time. The introspections recorded at the time of writing the test show the dependence of recall upon some form of imagery. "At first there is nothing more in the way of recall than a picture of a large page with rather dirty type, with Sec. No. 41 rather low on the left-hand side of the page. As



I hold myself to it, I think that the subject-matter must be of 'implication,' but with no memory of what phase of it may be considered. All is accompanied by unpleasant strain sensations. Nothing more comes. I give it up for a time. Nothing more would come yesterday. Occasionally a stray word would present itself, with the feeling that something else was behind it, but nothing definite would develop, and I could not be sure that the idea came from the right passage."

Summary, Subject I. (1) *Organic Chemistry*. (a) Distributed repetitions proved to be more effective than those which followed one another closely, but there seemed to be little difference in value in the spacing of the repetition. That is, material read each day for five days was remembered as well as that read three times the first day and twice the second day. Also material read twice in one day and three times on the following day was recalled as well as that read on alternate days. The results are too few in number to give any definite conclusions, except that when the subject-matter is to be tested in two weeks, repetitions which are scattered are more advantageous. (b) The questions brought back more than free recall. (c) There was a decrease with time in the amount retained.

(2) *Mathematics*. There were too few results to give any conclusions.

TABLE I  
SUBJECT I

Organic Chemistry		Repetition	Mathematics	
Free Recall	Question Recall		Free Recall	Question Recall
Tested in 2 Weeks				
19.9	19.9	5 times in one day.....	.....	.....
25.8	48.6	Daily for 5 days.....	6.7	14.1
42	52	Alternate days for 5 days.....	29	40
29.1	55	3 times 1 day, twice 2nd day.....	24.3	45
40	65	2 times 1 day, three times 2nd day...	0	28
Tested in 10 Days				
43.9	46.1	Daily for 5 days.....	.....	.....
Tested in 7 Days				
		Daily for 5 days.....	14.8	24.4
Tested Following Day				
65	74	5 times in 1 day.....	.....	.....
72.7	83.3	Daily for 5 days.....	32.6	36.7

Table II, Subject II. (1) *Embryology*. These assignments were short, owing to difficulty in terminology. The approximate time for reading was from 5 to 6 min. Material read once each day for five days and tested in two weeks gave the highest percentage of any of

the two weeks' tests. One introspection may be noted, showing the subject's dependence on visualization. "I can not remember much about this material. I could not visualize; it has no meaning for me because I can not picture it, or connect it with anything in my experience."

(2) *History*. The history read once every fifth day gives a very low percentage, showing that repetitions so far apart are probably past the limit of advantageous dimension. One introspection may be quoted: "This history I did not feel nearly as sure of as I did the first. I have a general sort of hazy notion; can get but very few facts, and they seem vague and unconnected. During the learning process, I was conscious of the confusion of the details of the material with that of the first. Had I been able to go back over it, I think I should have been able to keep it straight."

The portion read five times in one day and tested on the following gave a test that was an almost exact reproduction of the text, including figures and number of troops. This was very unusual. It may be noted that even after two days, with the additional help of the questions, there was a fall of 5%.

Summary, Subject II.<sup>2</sup> (1) *Embryology*. The results were too few in number for any positive conclusions.

(2) *History*. (a) Repetitions distributed within limits (up to one repetition a day for five days) proved most effective for recall when the subject-matter was to be tested in two

TABLE II  
SUBJECT II

History		Repetition	Embryology	
Free Recall	Question Recall		Free Recall	Question Recall
<i>Tested in 2 Weeks</i>				
31.1	40.5	Daily for 5 days.....	43	75
0	43.4	.....	.....	.....
....	....	3 times 1 day, twice 2nd day.....	0	10
....	....	2 times 1 day.....	.....	.....
....	....	2 times 1 day, 3 times 2nd day.....	25	40
4.12	10.9	Once every 5th day.....	22.2	.....
31.9	40	Alternate days for 5 days.....	15.7	27.7
<i>Tested in 7 Days</i>				
47.6	54.3	Daily for 5 days.....	....	....
<i>Tested Following Day</i>				
98.6	92.3	5 times in 1 day.....	65.1	65.1
70.8	72	Daily for 5 days.....	65.1	65.1

<sup>2</sup> These subjects, I and II, worked irregularly and the series is not complete. The results, however, show the tendency, confirmed by the more complete series which follow, for the scattered repetitions to have a greater value than the accumulated when the matter is recalled after the longer periods of time, while the two are of approximately the same value for recall after a single day.

weeks. (b) For immediate recall the accumulated repetitions proved more valuable than distributed. This, however, was in a single instance only. It shows the tendency, confirmed by the following series of other subjects, that for immediate use the value of the scattered and of the massed repetitions is about equal. (c) The questions brought back more than free recall. (d) There was a gradual decrease with time of the amount retained.

Table III, Subject III. (1) *Embryology*. The assignment read five times in one day and tested in two weeks gave the following introspection. "I can not recollect distinctly what the material was about. I studied it in succession I think, but I don't remember what it is on. I may be able to answer some of the questions when I see them." It is interesting to note, in relation to this, that the portion read once every fifth day and tested in two weeks gave almost exactly the same percentage and about the same introspection. "Have tried to think just what this section is about, but I can't place it. I have a mass of facts before my mind but I can not pick out the pertinent ones." Repetitions which come too close together or too far apart are of little value. There is utter blankness when trying to recall the subject-matter. Only when some form of imagery is possible does any of the content return.

Another introspection may be noted. "I am completely mystified as to the exact content of the paper I am supposed to reproduce to-day. Not that I do not know some of the facts, but I can not distinguish which they are. I think I shall be able to answer some of the questions without difficulty." When the questions were answered, the following comment was made. "This is a terrific paper, but I can not account for it exactly. Maybe it is due to overwork, or I was not in proper shape when I studied it."

Summary, Subject III. (1) *Embryology*. (a) Distributed repetitions are more effective than accumulated, without exception, in the two weeks tests. One reading a day was the most effective distribution. In the material read on alternate days, the initial test with free recall is absent, but the question recall shows the highest percentage of material retained in the two weeks series. (b) Repetitions too close together or too far apart are of little value. (c) For immediate recall, accumulated repetitions were as valuable as distributed. (d) The questions brought back more than free recall. (e) There was a decrease with time in the amount retained.

Without exception, with Subject III, distributed repetitions proved more effective for recall after intervals of seven days to two weeks.

(2) *Geology*. The tests were too few in number for a comparative summary. The two points which were shown are: (a) Questions brought back more than free recall; (b) There was a decrease with time in the amount of the material retained.

TABLE III

SUBJECT III

Geology		Repetition	Embryology	
Free Recall	Question Recall		Free Recall	Question Recall
<i>Tested in 2 Weeks</i>				
33.3	22.2	Daily for 5 days.....	26.6	46.6
....	....	5 times in 1 day.....	0	16.6
13.7	35.5	Alternate days for 5 days.....	....	57.1
....	....	4 times 1 day, once 2nd day.....	0	24.2
....	....	Once every 5th day.....	....	16.3
....	....	3 times one day, twice 2nd day.....	0	25
<i>Tested in 10 Days</i>				
13.8	33.3	Daily for 5 days.....	....	....
35.9	....	2 times 1 day, 3 times 2nd day.....	61.9	....
....	....			
<i>Tested in 7 Days</i>				
....	....	Daily for 5 days.....	38.4	42.1
....	....	Once 1 day, 4 times 2nd day.....	0	33.3
<i>Tested Following Day</i>				
61.7	67.6	5 times in 1 day.....	47.7	63.1
....	....	Daily for 5 days.....	40	40
40	40	3 times 1 day, twice 2nd day.....	....	....
<i>Tested in 2 Hours</i>				
47.2	41.7	Daily for 5 days.....	85.7	....
48.7	43.6	....	61.9	71.4
....	....	....	53.5	50

Table IV, Subject IV. (1) *Embryology*. As has been stated, embryology was entirely new subject-matter; the names and terminology were difficult and the sections, therefore, short. This was the same material used by Subject III. An introspection at the time of testing material read three times in one day and twice the second day is as follows. "I had a perfect picture of all of this at the time of reading. It has completely faded. This was one assignment I felt sure of, but the test shows almost a complete loss of material." After six weeks the questions were answered and 40% was retained, showing that the subject was justified in her feeling of surety.

(2) *History*. I shall quote this subject's introspections in detail, since they give clearly her method of learning and her dependence, for recall, upon a definite image. "In learning the material, I tried to visualize the whole, that is, to put it into an outline form and build up relations between the parts so that one would naturally call up what followed. I also pictured out very accurately every detail. In the recall I found that these two things helped me. It was only after I could not get a mental picture of the details that I could not remember. In recall, I found I was more or less dependent upon my outline. Just as soon as the main facts came to me, the rest fell in order. It seemed to be a continuous process without effort. In many

cases I could nearly get the facts. I could feel them on the fringe and yet they were inhibited. I could see just how they stood on the page, but just what they were I could not get. In several places there was confusion; several things came back, but I did not know their relation to each other or to the whole. I felt that if I could get the name I could fill in the detail. In a few cases whole areas were blank. I could not get a visual picture of the page or fact. The whole mass had fallen away. I knew something was gone, but just what or where I could not tell."

Summary, Subject IV. (1) *Embryology*. (a) Distributed repetitions proved more effective than accumulated repetitions, without exception, particularly in the two weeks tests. One repetition each day and one repetition on alternate days were most advantageous. There was little difference in the amount retained when the repetitions were divided in various ways, as three times in one day and twice the second day, or four times in one day and once the second day. Likewise, in the ten and seven day tests, daily repetitions were more valuable than those which were made all in one day or two or three a day. (b) For immediate recall, cumulative repetitions were as valuable as distributed. (c) Repetitions too far apart, as one repetition every fifth day, as well as those too close together, as five in one day, were of little value. (d) The questions brought back more than free recall. (e) There was a decrease with time in the amount retained.

(2) *History*. (a) Distributed and accumulated repetitions proved to be about equally valuable, for both long and short intervals. This is an exception to the rule which has seemed to hold up to this point. The embryology experiments of Subject IV showed, without exception, the advantage of distributed repetitions for recall after long intervals. One possible explanation for the equal effectiveness of accumulated repetitions in this subject-matter might be that the subject had an unusual ability in remembering historical material. She seemed to be able to reproduce it no matter how it had been read. (b) For immediate testing, scattered and massed repetitions were of equal value. (c) The questions brought back more than free recall. (d) There was a decrease with time in the amount retained.

One review test may be noted, in passing, by Subjects III and IV. They were given on February 26th material they had read the latter part of October and the first of November, and on which they had been tested at that time. The conditions of the review experiment were that they were to reread the original text and test in two hours. The four assignments which were retested in this way showed that, after



several months, the subject-matter first learned with repetitions distributed within the limits of one repetition every fifth day showed a higher percentage of material retained.

TABLE IV

SUBJECT IV

History		Repetition	Embryology	
Free Recall	Question Recall		Free Recall	Question Recall
<i>Tested in 2 Weeks</i>				
23	42.3	5 times in 1 day.....	0	27.7
32.5	46.3	Daily for 5 days.....	7.1	64
45.3	49.4	Daily for 5 days.....	60	66
23.2	61.3	Alternate days for 5 days.....	43.4	52.3
33.3	69.8	3 times 1 day, twice 2nd.....	25	40
16.2	50	Twice 1 day, 3 times 2nd.....	0	5
....	....	Once every 5th day.....	0	30
....	....	4 times 1 day, once 2nd day.....	24.2	24.2
<i>Tested in 10 Days</i>				
52.6	59.4	Daily for 5 days.....	36.8	88.4
34.3	62.5	Twice 1 day, 3 times 2nd.....	19	14.5
<i>Tested in 7 Days</i>				
59	49	Daily for 5 days.....	52.5	84.2
54	25.5	Daily for 5 days.....	52.3	66.6
54	25.5	Daily for 5 days.....	52.3	66.6
....	....	5 times in 1 day.....	28.5	45.6
<i>Tested Following Day</i>				
56.9	62.8	5 times in 1 day.....	61.1	88.8
82.6	64.2	Daily for 5 days.....	20	16
52.8	64.3	3 times 1 day, twice 2nd.....	....	....
<i>Tested in 2 Hours</i>				
63.4	65.3	Daily for 5 days.....	78.5	78.5

Table V, Subject V. My own experiments fall into two well-defined groups. The first group consists of tests made in fourteen, ten, seven, four, and two days, and two hours, with the five repetitions divided differently, as three times one day, two times the next, or four times one day and once the following day, etc.

The results of the other subjects, as well as my own, seemed to show the advantage of divided repetitions over longer periods of time. Believing this point to be worthy of further investigation, I undertook the second portion of this experiment. A series of 60 tests was planned. This series was made up of 10 tests of five times in one day, and 10 tests every day for five days, tested the following day, in two weeks, and one month. I made up the questions for myself many weeks, often several months, before I was ready to use the material. I made the sections to be read shorter and, as has been said, I used various books and pamphlets on economics, thereby making it possible

to have many tests under way at the same time. In this way the second series of the 60 tests was carried on more systematically and with less loss of time than the first series.

*First Series of the Worker's Experiments*

(1) *Economics.* In my introspections I find that, almost without exception, I laid stress upon the necessity of visualization for recall. At first this was more or less unconscious. I did not realize my dependence upon it. One of the comments upon an economics assignment was: "I can visualize where what I don't know is. It is over the page, all of that page, and the top of the next, but I can not get it."

Comment may be made in passing upon one test of material read every day for five days and tested in ten, which gave only 12% in the initial recall. When I made this test, I was ill with tonsillitis. Any state of imperfect physical or mental health or fatigue greatly influences the amount recalled of material which has been learned, and explains departures from the general rule. In this connection there is this introspection; "I simply can't get it at all. This assignment was read five times in succession late at night after a busy day. I know where my last assignment stopped. I am curious to see the questions, for I feel I know some of the material if I could only start, but I have no visualization of any portion of it. I remember exactly where on the page the last assignment stopped, and something of its content, but what follows is an absolute blank." It is interesting to note that the above-mentioned last assignment had been read on five successive days and tested in two weeks. Thus, after several weeks, it was retained better than the later assignment read five times in one day and tested in four.

(2) *History.* The introspections written at the time of reading or of testing the history assignments show the same dependence on visualization for recall. One comment was: "I have lost all visualization here; that is, the continuity is gone, and these isolated facts have returned without any logical connection."

There was one three-weeks test in this group, which gave 3.8% with free recall and later 44% with the questions. The note accompanying the first test was: "I think if I could only start, I could remember it." Evidently this was true, as the questions two days later showed.

After four weeks, material read three times in one day and two times the following day gave very low percentages. Compared with history read in the same way and tested in two weeks, giving 14.6%—25.3% as compared with 9.3%—12.8% in this test, there was but relatively little difference. Little was lost between two weeks and one month.

Several other introspections may be noted which are general in their nature, and summarize in a way the introspections of this portion of the experiment. "After the assignment was read, it was dropped completely out of mind. So completely that often there would be no idea of what the subject-matter was about when the time came to test the material read, for instance, two weeks before. Then, by long continued thinking, the place where the assignment began came into consciousness, that is, on what part of the page. Next, the ideas in the text came, very often many of the same words if the test was an immediate one, but by no means verbatim if the

interval had been a long one since the last reading. When the continuity was lost (and this continuity seemed strangely dependent upon the visualization of the subject-matter), that is, when the logical connection was gone, everything seemed to slip away. Isolated facts might come up, but without their connection. There is nothing quite comparable to the feeling of utter blankness, and of strain as well, in attempting to recall an assignment after a long period of time for which the repetitions had been cumulative. There is present a realization that some of the subject-matter is retained, but what, it is impossible to tell, for it was not forcibly enough imprinted to be revived."

*Summary of the Writer's First Series of Experiments*

The full series of experiments in both economics and history shows the same general tendency throughout. The following summary will, therefore, be based upon the results of the work done in both subjects. For both economics and history, we may say that:

(1) Distributed repetitions proved more valuable than accumulated repetitions for recall, particularly after seven, ten, and fourteen days. One repetition each day and one on alternate days were the most effective. (2) Accumulated repetitions were as effective as scattered for immediate recall. There was no great difference in the amount recalled when the repetitions were divided differently. (3) The questions brought back more than free recall. (4) There was a decrease with time in the amount retained.

TABLE V  
SUBJECT V, FIRST SERIES

History		Repetition	Economics	
Free Recall	Question Recall		Free Recall	Question Recall
<i>Tested in 2 Weeks</i>				
36.5	36.5	Alternate days for 5 repetitions.....	22.2	33.3
14.6	25.3	3 times 1 day, twice 2nd.....	11.7	33.3
0	28.8	Twice 1 day, 3 times 2nd.....	17.4	30
<i>Tested in 10 Days</i>				
44.8	49	Daily for 5 days.....	12.3	38.4
42.5	....	Daily for 5 days.....	....	....
18	38	3 times 1 day, twice 2nd.....	20	28.5
....	....	3 times 1 day, twice 2nd.....	0	32.4
....	....	Once 1st day, 4 times 2nd.....	28.5	46.4
25	33	5 times in 1 day.....	24.2	36.3

TABLE V—Continued  
SUBJECT V, FIRST SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
<i>Tested in 7 Days</i>				
46.8	59.3	Daily for 5 days.....	54	25.5
5.7	30.7	Daily for 5 days.....	36.1	44.9
34.6	38.4	Once 1st day, 4 times 2nd.....	32.2	41.9
35.6	43.8	3 times 1 day, twice 2nd.....	30.4	30.4
		5 times in 1 day.....	20	33
<i>Tested in 4 Days</i>				
46	52	5 times in 1 day.....	0	21.6
43.3	52.8	Daily for 5 days.....	47.3	47.3
51.8	62.5	3 times 1 day, twice 2nd.....	25.6	42.8
48.2	54.2	Once 1st day, 4 times 2nd.....	39.5	55.8
58.8	64.7	Once 1st day, 4 times 2nd.....		
21	40	Twice 1 day, 3 times 2nd.....	38.4	53.8
<i>Tested Following Day</i>				
52.8	65.5	3 times 1 day, twice 2nd.....	67.5	52.5
		3 times 1 day, twice 2nd.....	40	46.6
47.1	54.5	Once 1st day, four times 2nd.....	52.5	56
52.1	52.1	Once 1st day, four times 2nd.....		
		Daily for 3 days, 1 day omitted and 2 succeeding days.....	41.9	54.4
		Every alternate day for 5 repetitions, 2 days between last 2 readings....	46.1	46.1
54	56	4 times 1 day, once 2nd.....		
58.1	66.6	Alternate days for 5 repetitions.....	47.5	55
		Alternate days for 5 repetitions.....	52.9	52.9
58.8	68.8	Twice 1st, twice 2nd, once 3rd.....		
<i>Tested in 2 Hours</i>				
76.7	76.7	5 times in 1 day.....	67.7	67.7
70.3	79.2	Daily for 5 days.....	56.9	54.9
64.5	56.1	3 times 1 day, twice 2nd.....	65.7	60
<i>Tested in 3 Weeks</i>				
3.8	44.2	Twice 1 day, 3 times 2nd.....		
<i>Tested in 4 Weeks</i>				
9.3	12.8	3 times 1 day, twice 2nd.....		

*Second Series of the Writer's Experiments*

The particular noteworthy point which had been developed thus far was the greater value of distributed repetitions in learning, when the subject-matter was to be tested after a long interval. The following experiments were performed in an attempt to develop this hypothesis into a theory. A series

of ten experiments each, of five repetitions in one day and five repetitions, one each day, tested on the following day, in two weeks, and one month was performed, making a total of 60 tests (or 120 in all, including the answers to the questions).

The results show a marked degree of consistency. All the figures which I shall use to compare one series with another will be averages of each series of ten experiments. For example, 11.49%—22.41% means that the average amount recalled for the ten tests, five in history and five in economics, for free recall was 11.49%, and with the aid of the questions, 22.41%.

(1) The first comparison of averages in this series will be that of material read five times in one day, and of material read each day for five days, both tested in one month.

Read	Tested	Average (10 tests)	Mean Variation
5 times in 1 day	1 month	11.49—22.41	4.8 — 6.48
Daily for 5 days	1 month	30.59—41.66	9.41—10.84

That is, almost three times as much was recalled after the end of a month, when the repetitions were divided. Almost twice as much was recalled with the aid of the questions when the repetitions were divided.

(2) The second comparison is as follows. The time of testing in this case was two weeks after the last reading.

Read	Tested	Average (10 tests)	Mean Variation
5 times in one day	2 weeks	13.13—30.2	10.21—10.57
Daily for 5 days	2 weeks	37.26—49.09	11.24—11.95

The relation is much the same as in the tests of one month; almost three times as much was retained when the repetitions were scattered. There is not so much difference in the amount retained with the questions as in the preceding case, although appreciably more was recalled with the questions when the repetitions had been scattered.

(3) The third comparison of this series is of the averages of subject matter tested the day after reading.

Read*	Tested	Average (10 tests)	Mean Variation
5 times in 1 day	Next day	66 — 69.5	10.62— 8.87
Daily for 5 days	Next day	64.4—70.6	10.57—10.19



For immediate recall, cumulative repetitions proved as effective as distributed. All of the subjects in this experiment have shown the same result, indicating the tendency for accumulated repetitions to be as effective as those which are scattered. There are always many factors which might influence a single test, and make variable the amount retained. This series is significant in that it represents averages, and therefore confirms the point that repetitions which follow one another closely are as valuable as those which are distributed, if material is to be utilized at once.

The introspections of this series have the same tenor as those of the former series and of the other subjects of the experiment. I shall quote only a few. Of the history assignments tested in one month we find: "I can see where the assignment is on the page even to the paragraphs; I know that if I can ever start, some facts will come back to me. I have no idea as to the words the author used; everything is general. I know just where a long succession of facts in the text is; if I only had the right cue to set me off, I am sure I could reproduce some of them. But it is all exasperatingly faint." I quote this because it is typical of many of the introspections written at the time of testing assignments read a month before. Again, "I have entirely lost all the wording of this; the sentences were rather long as I recall them, and only a few of the main ideas come, clothed in my own words."

Another comment was: "I have a general idea of the whole, but the several Russian names throughout the portion confused me. While learning it, I remember thinking, 'Now if I can't recall those names, I won't recall any of it, and I am afraid I can't recall them.'" It seems to have worked out so.

A general observation of the material read five times in one day and tested in one month is: "Invariably there is a decided vagueness, and if anything is recalled it is only in the most general terms. I have a visual picture of the whole assignment, and although I can see where it begins and ends, I have but little idea of the content."

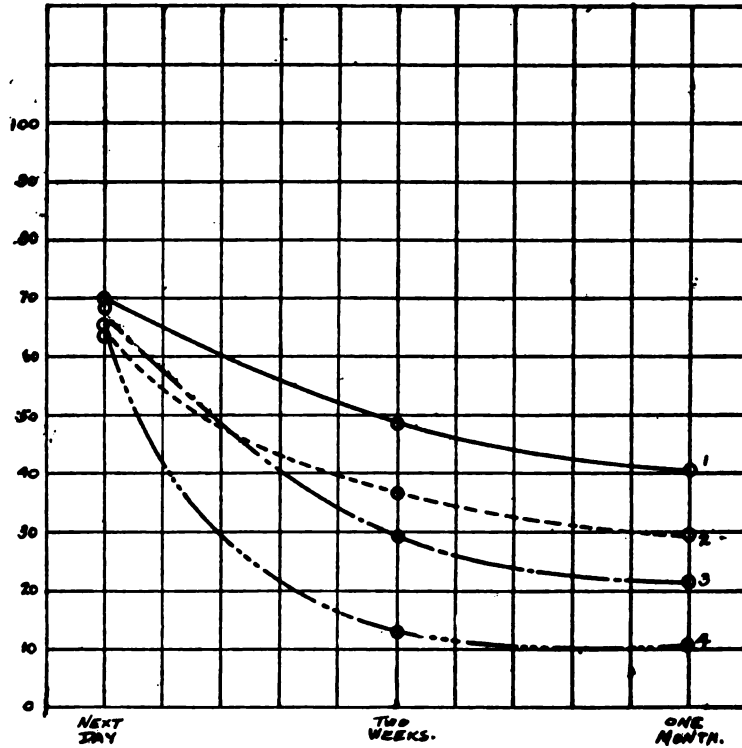
Similarly, of a test five times in one day and tested in two weeks, we find: "I can see this whole paragraph but I know absolutely nothing in it." Again, "I know that this assignment began on the right-hand side of the page about the middle, but I simply cannot recall anything." Very careful and full introspections were taken, but it is sufficient to quote these which I have chosen as typical of them all.

I do not know how I knew what the general idea was when I could not recall it. It is significant that I had to have, first of all, the visual picture of the appearance of the page for any recall; then came the feeling that I knew something of the subject-matter in the text. Yet I could not bridge the gap between my concrete image of the printed page, which I had once known, and the general idea of its content.

### *The Curve of Forgetting*

The writer's second series of experiments contains data sufficient to show the influence of time upon the amount of material retained.

CURVE I.



## LEGEND.

1. MATERIAL READ EVERY DAY FOR FIVE DAYS.  
QUESTION RECALL —————
2. MATERIAL READ EVERY DAY FOR FIVE DAYS  
FREE RECALL - - - - -
3. MATERIAL READ FIVE TIMES IN ONE DAY  
QUESTION RECALL. — · — · — · — · — · —
4. MATERIAL READ FIVE TIMES IN ONE DAY  
FREE RECALL — · — · — · — · — · —

## SUBJECT MATTER READ FIVE TIMES IN ONE DAY

	Tested	Free Recall	Question Recall
Following day.....	66	% retained	69. 5% retained
In 2 weeks.....	13.	3%     "	30. 12%     "
In 1 month.....	11.	49%     "	22. 41%     "

## SUBJECT MATTER READ FIVE TIMES IN ONE DAY

	Tested	Free Recall	Question Recall
Following day.....	64.	41% retained	70. 59% retained
In 2 weeks.....	37.	26%     "	49. 69%     "
In 1 month.....	30	%     "	41. 66%     "

The accompanying curve shows this result graphically. It conforms with the work done on the Curve of Forgetting. Forgetting occurred rapidly at first, and then relatively little was forgotten in the period from two weeks to one month. Forgetting occurred much more rapidly when the material had been learned with the repetitions massed, for the first two weeks, but after that the loss was slight. There was a much greater drop in the curve when the repetitions were accumulated. No matter in what way the material was learned, the loss in the first two weeks was very rapid. It is for retention after longer intervals that the value of divided repetitions is particularly noticeable. We may say, then, that for these experiments, Ebbinghaus' Curve of Forgetting applies to sense as well as to nonsense material.

*Class Experiments.*—The writer made some experiments on a class in Introductory Psychology. The results of four written lessons show the same general tendency we have found throughout; namely, that, for immediate recall, readings following one another closely are as effective as readings which are scattered. The method used was this. The instructor assigned a certain number of pages in the text-book in Elementary Psychology. He asked one half of the class, in preparation for their twenty minute written lesson, to read the assignment three times in succession, the other half to read it on three alternate days. One hundred eighty-five papers written by students who conformed to the instructions were obtained, despite the failure of some of the students to follow directions accurately.

The ideas in the text were scored as has been described, and the average number of ideas that could be reproduced for each group was found.

TABLE V

## SUBJECT V, SECOND SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
<i>Tested in 1 Month</i>				
10.5	26.3	5 times in 1 day.....	18.7	31.2
0	15	5 times in 1 day.....	0	16.6
10.5	15.7	5 times in 1 day.....	11.7	23.5
13.6	27.2	5 times in 1 day.....	9	27.2
21	31.5	5 times in 1 day.....	9.9	9.9
Average for free recall for both subjects 11.49, M. V. 4.8				
Average for question recall for both subjects 22.41, M. V. 6.48				
40.9	45.4	Daily for 5 days.....	41.1	58.8
18.7	31.2	Daily for 5 days.....	26.6	33.3
26.3	31.5	Daily for 5 days.....	20	33
22.2	38.8	Daily for 5 days.....	22.7	31.8
31.2	37.5	Daily for 5 days.....	56.2	75
Average for free recall for both subjects, 30.59, M. V. 9.41.				
Average for question recall for both subjects, 41.66, M. V. 10.84.				
<i>Tested in 2 Weeks</i>				
9	32.7	5 times in 1 day.....	0	20.5
0	20	5 times in 1 day.....	0	7.1
31.5	42.1	5 times in 1 day.....	24.4	44.4
22.2	33.3	5 times in 1 day.....	26.6	40
0	20	5 times in 1 day.....	17.6	41.1
Average for free recall for both subjects, 13.13, M. V. 10.29.				
Average for question recall for both subjects, 30.22, M. V. 10.57.				
20	32.5	Daily for 5 days.....	40	50
47.3	63.1	Daily for 5 days.....	64.7	64.7
40	66.6	Daily for 5 days.....	50	61.1
25	33.3	Daily for 5 days.....	17.6	23.5
34.7	52.1	Daily for 5 days.....	33.3	50
Average for free recall for both subjects, 37.26, M. V. 11.24.				
Average for question recall for both subjects, 49.69, M. V. 11.95.				
<i>Tested Following Day</i>				
55.7	55.7	5 times in 1 day.....	54.6	62.5
58.4	66	5 times in 1 day.....	51.3	51.3
90	85	5 times in 1 day.....	76.4	76.4
73.9	73.9	5 times in 1 day.....	60	73.3
72.2	83.3	5 times in 1 day.....	67.7	67.7
Average for free recall for both subjects, 66, M. V. 10.62.				
Average for question recall for both subjects, 69.5, M. V. 8.87.				
53.3	66.6	Daily for 5 days.....	49.3	49.3
49	63.4	Daily for 5 days.....	77.7	83.3
70.5	76.4	Daily for 5 days.....	61.9	66.6
62.5	62.5	Daily for 5 days.....	94.4	100
57.1	64.2	Daily for 5 days.....	68.4	73.6
Average for free recall for both subjects, 64.4, M. V. 10.57.				
Average for question recall for both subjects, 70.6, M. V. 10.19.				

"A" is material read three times in succession.

"B" is material read on three alternate days.

The table showing the results from these tests is as follows:

Quiz	No. of Papers	How Read	Average % Retained
I.....	17	A	34
	15	B	46
II.....	25	A	45.7
	24	B	45.6
III.....	34	A	49
	19	B	52.4
IV.....	32	A	40.2
	19	B	39.3

These results conform with the general tendency which has been shown in the experiments of the subjects tested at length, namely, that readings which are distributed and readings which are accumulated are equally effective for immediate recall.

*Brief General Summary of the Results of All the Subjects of the Experiment*

(1) Distributed repetitions were more valuable than those which were accumulated when the subject-matter was to be tested in two weeks or one month.

The experiments of Subject I in Organic Chemistry and Subject II in History, though few in number, followed this rule, and, without exception, the Embryology tests of Subject III. (The Geology experiments of Subject III were too few in number to be significant.) The full series of Embryology experiments of Subject IV followed the same law. An exception is to be found in the History experiments of Subject IV, in which accumulated and distributed repetitions were of equal value. The single experiments in the first series of Subject V showed the advantage of scattered repetitions, yet not so strikingly as the second series. In the latter, without any exception, distributed repetitions proved to be very much more valuable, for periods of two weeks and one month. Since the final percentages of this series are based upon averages, not single experiments, we may attach much more importance to them.

(2) For immediate recall (recall after two or twenty-four hours) accumulated repetitions were as valuable as distributed.

This was found to be true throughout the experiments for all subjects and for the tests made upon the class in Elementary Psychology.



(3) Repetitions too far apart were as ineffective as those which came too closely together. Although there were relatively few of the tests with repetitions distributed to one every fifth day, the results which were obtained showed that repetitions so far apart were almost valueless.

(4) The questions always brought back more than free recall.

(5) There was a decrease with time in the amount retained.

(6) For all subjects, there was a dependence for recall upon visualization of the text.

#### IV. DISCUSSION OF RESULTS AND SUMMARY

##### (1) *Discussion of Results*

(a) *Sources of Error.* The sources of error which form an important factor in such an experiment as this should first be noted. Most important of all, perhaps, is the "personal equation," the individual variation in each subject from day to day, in general health, fatigue, mood, interest and lack of interest. This is an element which cannot be controlled experimentally. A passing mood, or a state of fatigue at the time of learning or of testing, will entirely alter the usual result.

Another source of error which cannot be controlled is the lack of perfect uniformity in the difficulty of the material to be learned. The same books and pamphlets were used throughout the experiment, but as in all subject-matter some pages were easier than others. They had to be taken in succession, otherwise the logical connection would have been lost. Thus if a few pages happened to be easier to remember and the repetitions to be distributed advantageously, an unusually high per cent was retained. This lack of absolute uniformity in the subject-matter was a constant factor, and can only be eliminated by averaging a number of results.

It may be said that knowing the time the test was to be made might introduce a source of error. Whether it was to be made in two hours or in two weeks might, unconsciously, influence the amount of effort expended in learning. Under ideal conditions of experiment, the subject would not know when he was to be tested. Practically, however, it was necessary to map out the work in advance, and the subject was given explicit directions when to read and when to test, for a short series of experiments at a time. In my own later experiments, when I had several tests overlapping, I paid but very little attention, if any, to the time of testing, while I was reading the assignment.

Another criticism, and perhaps a just one, is that I scored the text, test, and made out the questions on my own material. This may appear to have given me an unfair advantage in having seen the subject-matter before beginning to learn it. I do not think, however, that this was the case. The questions on the subject-matter were made out for other subjects as well as for myself, often months before they were to be used. As a rule, I did not even recognize the material when I read it.

Again, I scored all material myself; it was not rechecked by anyone else. I made two checks to see if this criticism was valid, and to see if my method of scoring was consistent. First, I scored again some tests of my first series which had been made a year or more before, and secured practically the same percentages. Secondly, when I began my experiments again, after an interval of over three years, during which time the problem was not touched, not even thought of, I compared the results with those obtained before the work was interrupted, and found a general uniformity. These two checks seem to indicate that the criticism has no vital significance.

*(b) Statement of Results and Their Practical Application*

One of the points to be considered first in a discussion of the results is this. All the subjects without exception, some to a far greater degree than others, were dependent upon visualization of the subject-matter for reproduction. There was in no case recall of the logical sense of the subject-matter without some imagery. This is in harmony with Finkensbinder's (5) investigations. He found with 18 subjects using problems of various kinds and of varying degrees of difficulty, as mathematical problems, mechanical puzzles and the like, that the visual image was the first definite figure to appear in 95% of the recalls. In this connection Offner(13) says (in relation to reproduction) that we have the factor of the appearance of the text, having a certain association of words in a spatial scheme.

Since visualization was essential for any reproduction, the results of my experiments agree with those of the investigators who hold that there must be some imaginal factor for recall, as opposed to those observers who hold that imagery is not essential. We can only conjecture as to the dependence upon imagery had some of the subjects for this experiment been of a non-visual type, as the acoustic-motor type, for example.

It is interesting to note, as has been stated before, that there were few errors in the tests which were scored. Gross

errors were seldom made. As a rule, if the ideas in the text were retained, they remained in their correct form. In the immediate tests, the author's style and his actual wording were sometimes reproduced. As the time-intervals for testing increased, however, the important ideas remained and became more generalized, the original wording of the author was lost, and more and more details dropped out. This also coincides with the general results of memory investigations in logical material, particularly with those of Henderson. We also found a tendency to generalize.

One of the most important results of this investigation is to show that for immediate recall accumulated repetitions are as valuable as divided. This harmonizes with general observations. It is a common experience for us to learn something for immediate use; say, to be utilized in a few hours or on the following day. School children learn for immediate reproduction; the more quickly the content is reproduced the more nearly accurate it is. Öffner(13) asks: "May the greater value of accumulated repetitions for recall be attributed to our habits of learning, or are our habits of learning due to this?"

Accumulated repetitions make material available for immediate use; in common parlance this is really "cramming." An example in a superlative degree is the actor's cramming or "winging" of a part. "A part taken in the morning can be done perfectly at night; every scene was read before going on, the shape of the part was mechanically fixed in memory, that is, the position of each speech on the paper. Words would be acquired but not always the sense, for there would be no time to think about the context . . . . One result of these hurried studies was, words would not remain, fading from the memory as rapidly as acquired."<sup>3</sup>

The same principles apply in a lesser degree to "cramming for examinations." Lessons which were not learned at the time they were assigned can be so intensively studied that, for 12 or 24 hours, the greater part of the work of the semester can be retained. It is a common observation that anything so learned vanishes almost immediately.

The facts in practice, then, are that for immediate use a great mass of material can be made available by intensive review or "cramming," a short time before it is to be utilized. Our experiments upon subject-matter such as is used in practical life, carried on as accurately as possible, showed exactly the same conditions to be true. We may conclude, therefore,

<sup>3</sup> H. F. Osborn, Rapid Memorizing, *Psych. Rev.*, ix., 1902, 183 f.

that subject-matter which is studied very intensively a short time before it is to be used, both in practical life and in scientific investigation, is soon lost, and very little if any of it can be recalled after a lapse of time.

The second important result of this research is the fact that accumulated repetitions, which are highly effective for immediate recall, are ineffective for recall after several weeks. Our experiments show the very great advantage of divided repetitions when learning is to be tested after longer time intervals, particularly two weeks and longer, after the last reading.

While our experiments cover too few periods of time to give a complete picture or to formulate an equation, our results for all of the subjects of the experiment show that forgetting took place rapidly at first, and then much more slowly. Meumann says that whatever we know twelve or more days after learning presumably stands longer at our disposal. On the other hand, what we can reproduce at a glance or at a single reading, we have for only a short time. Similarly in our experiments, there was a rapid loss at first, while if the subject-matter were retained two weeks there was little additional loss at the end of four weeks. Our few results indicate that the curve of forgetting has the same general form for logical as for nonsense material.

The fact that divided repetitions have an advantage over accumulated has been explained in two distinct ways. The first suggestions were based on the assumption that the distributed repetitions had some advantage for learning inherent in the distribution itself, that there was less fatigue or ennui or other disturbance, when few repetitions were made at one time. A second hypothesis is that the distributed repetitions offer opportunities for some favorable change in the effects of learning, some change that takes place after the learning gives the distributed repetitions an advantage. Each of these general classes offers several subordinate suggestions. The first suggestion of the first class was made by Müller, to the effect that the fatigue from many repetitions in succession would explain the more favorable results from the distributed repetitions. Under Müller's direction Jost made a series of tests in which a number of repetitions of other series were made before each of the repetitions with distributed learnings that should make the total number equal to the number given to the accumulated repetitions. This insured that the subject should be more fatigued or bored during the divided than during the accumulated repetitions. Neverthe-



less the advantage of the divided repetitions persisted. Müller seems to have accepted Jost's results as convincing evidence that his suggestion did not furnish the real explanation.

Another explanation in terms of conditions incident to the nature of the individual has been suggested recently for animals. Animals, too, learn a maze much more quickly with divided than with accumulated repetitions. In testing the phenomenon Lashley (10) confirmed the results of earlier investigators objectively, but attributed the fact to the dislike of being taken away from the food, and to general handling. This was an immediate accompaniment of reaching the food-box in accumulated repetitions, but was not so prominent in the divided repetitions. He is particularly vigorous in his preference for this as opposed to any 'setting' process in the nervous system as an explanation of the advantage for the divided repetitions. Whatever may be the status of this explanation for animal learning, there is no corresponding advantage for distributed repetitions in human learning. As Jost's experiments show, fatigue or ennui is not a sufficient explanation. One factor that might be expected to favor accumulated repetitions may be suggested. This is the evidence of making progress in the learning. As several repetitions are made at one sitting one understands, and feels in consequence a confidence in the ability to repeat, that is pleasant as a sign of progress. To be sure one does not actually know that one is learning. The only direct evidence of that is furnished by the test itself, but confidence of that ability increases with understanding. Were any detrimental influence of accumulated repetitions to exist in man, one would expect it to show itself in Jost's experiment mentioned above, in which the divided repetitions were repeated after a number of repetitions had been made of other series to induce the same state of fatigue.

The more usual explanation of the second type is in terms of the perseveration tendency. This must stand or fall with the interpretation of that term. If we assume with Müller that there is a continuance of the activity of the nervous system for some time, as long as forty-eight hours, and that during that time associations constantly increase in strength, we should have all that is essential for our purposes. In this assumption it is necessary to distinguish what might be called the immediate readiness for response from the persisting tendency to return that is connected with association. This may be made clear if we think of dispositions left in the nervous system as consisting of two parts; one, more temporary, which



gives the nerve tract affected a tendency to continue active for a relatively short time, and for this activity to become conscious whenever there is no other activity present; the other, more permanent, which serves in some way to connect that tract with another involved when the tract excited with it or immediately after it is aroused. This is the ordinary association. The former tendency dies away relatively quickly; the second persists in some degree for years and diminishes relatively slowly. Since the tracts are active in the same way during the perseveration period as during the original action, it may readily be assumed that the associative process or formation of connection between that and the tracts active during the process accompanying and immediately succeeding will be increasing in strength during the period of partial activity. If we grant this assumption, the greater effect of divided repetitions will be due to the fact that after each repetition the activity will continue for a time. With each repetition this activity will start at a maximum and will diminish to zero. When the same number of repetitions is made in immediate succession, the degree of activity of the tissues involved will not be much greater after the series than after each repetition, and its return to zero will be almost as quick as after each of the divided repetitions. What is gained for the divided repetitions as compared with the accumulated is the setting due to the perseveration for each of the groups of repetitions as compared with the single repetition.

The evidence for the existence of perseveration is, first, the general existence of a continuance of the cortical excitations as seen in the memory after-image. It is fair to assume that there is no sharp cessation of this persisting effect: the only question is how long it continues. Müller and Pilzecker, who first made use of the term perseveration, although the phenomenon had been observed by others before, cited the instances of songs that run in the head, the recurrence of vivid images of exciting positions in a game of chess, etc., which last for hours and occasionally days, as compared with the few seconds that the memory after-image has been demonstrated to continue. They also found that certain syllables shown in one experiment were likely to reappear in another experiment soon after, although there were no syllables shown, or other associations, that would suggest them. Foster, who repeated the experiments with the object of deciding whether these cases were sufficiently frequent to justify the assumption that perseveration was a factor to be reckoned with, came to the conclusion that cases of appearance of old syllables were

due to unnoticed associations, or to the fact that they were sounds likely to be made because of their ease or similarity to emotional expressions. It should be noticed that all that he claims is that they might be due to association, not that he was able to trace the associates in each case; and the ease or resemblance to emotional expressions is a matter of opinion. Foster also does not question the persistence of the nervous activity, but only the likelihood that experiences will rise to consciousness soon after their original appearance when not excited through associations. He grants all that is needed for the explanation of our facts.

The indirect evidence is also striking. I mean by indirect evidence in this connection a series of well established facts which can be readily explained if we assume perseveration, but which are hard to understand if we deny it. There are three such sets of facts: first, retroactive inhibition; secondly, the rapid forgetting for the first few hours or for two days, followed by a relatively slow decline of the strength of associations, and thirdly the fact we are dealing with, the advantage of divided repetitions. We can understand retroactive inhibition and the related retrograde amnesia of pathological states if we assume that the effects of experiences are at first of relatively little value for the formation of associations that shall lead to their reinstatement, and that, with the passage of time, through the perseveration they become more firmly established. The rapid course of forgetting of meaningless or nonsense material at first might also be explained if there are two parts to the memory process, one due to a quickly disappearing tendency to return, the other to a tendency that diminishes very slowly. The rapid diminution of the likelihood of recall at first would be due to the gradual dying out of the perseveration, while the later part of the curve would represent the more gradual weakening of associative bonds. Finally, we see that the perseverative tendency makes explicable the advantage of divided repetitions. Miss Perkins' results would also give an indication of the length of time the perseveration continues to be strong enough to be detected. Her results show that divisions up to one every third day were more effective than accumulated repetitions, and our present investigation confirms that for sense material. This agrees with the form of the curve of forgetting, which loses its sharp descent at the end of the second day, according to both investigations of the subject. We find, then, an explanation of the three important general facts and of the more detailed

time relations, if we assume that retention is partly due to perseveration, partly due to association.

Closely related to this explanation is Jost's suggestion that the determining advantage of the divided repetitions is to be sought in the fact that older associations are more easily brought to full effect by new repetitions, and also that older associations are loosened less quickly than more recently formed ones even if they have the same strength. This law was established before Müller and Pilzecker formulated their doctrine of perseveration, and so takes no account of the latter. It is easy to see that perseveration constitutes an explanation of the greater persistence and more ready revivability of the older associates. If we assume the two parts or phases of the retention process, perseveration and association, the associations would increase in strength during (and because of) the perseveration process. The tests made soon after learning would give a high percentage of successes by the method of successes, because part of the recall would be due to the perseveration. They would not be so quickly learned to the full, because the association process would not have attained the strength that it would have after the perseveration period has passed, and this alone apparently is of value, or at least is of greatest value, in learning to the point where the series can be repeated without promptings.

It is interesting to note that both in these experiments and in Miss Perkins' the distributions could be greater than were advantageous. We found no effect, or very slight effect, from repetitions made one each fifth day. Miss Perkins found that repetitions one each fourth day were less effective than one each third day. This might emphasize the second part of Jost's first law, that old repetitions are more easily brought to full strength than are new of the same strength. Taken with no other implication this might mean that the testing was of little or no value unless the associative trace was refreshed by a new repetition made soon after the perseveration had ceased. This is compatible with the perseveration theory, but one must admit the fact that the effect of the immediate repetition *plus* the setting is lost unless a new repetition is made within a certain time. The time must be before all effect of the first repetition is lost, and to obtain the best effect it must come when we may assume the perseveration of the original repetition is just disappearing.

Ladd-Woodworth(9) suggest that some of the effect of the division may be due to the more rapid forgetting of errors, which serves to leave the correct responses more prominent

than they are when the repetitions are accumulated. This would explain the more rapid learning of the mechanical operations of men and animals. It would be less important in rote or logical learning of verbal or symbolic material, in which few errors are made, and where the distributed form of learning is more effective even if each repetition is perfectly accurate. There it must be subordinate to some one of the other explanations.

On the whole it seems that there must be some change in the associations that takes place after each group of repetitions which is more favorable to the divided repetitions. In the light of Jost's law this also favors all older formed associations as compared with the new formed. The most likely source of this improvement in the period after each repetition is the perseveration tendency. The associations increase in strength during the perseveration, and if a new repetition is made before its strength has waned too much the effect of the distributions is beneficial.

We also need to explain the most striking general result of this investigation, that divided repetitions are relatively more favorable after a long period than after a short, after a week or a month than after twenty-four hours. Miss Perkins states that she had some evidence to this effect in her series of nonsense syllables. If we compare her results with those obtained by Jost we find that the difference between the published results of the two is not very great. Jost tested after twenty-four hours, Miss Perkins after two weeks. If we compare his results for 3x8 (eight repetitions on each of three days) with her 2x8, and his 6x4 with her 2x4, and his 2x12 with her 2x8, we find that he has 18, 39, and 53% for one observer and 7, 31, and 55% for another as against an average for all observers for Miss Perkins of 9, 25, and 43%. If we average the results of Jost's two observers there is a slightly greater difference in favor of the more distributed repetitions after the longer period, but it is so slight as to be negligible as compared with the differences we have found for sense material. That the difference should exist does not seem open to explanation from the facts of related phenomena, although it harmonizes with the observations of daily life.

An explanation of the fact of the greater effect of distribution after a considerable period than after a short might be found in the mere average time that elapses between learning and test. For if one repeat once a day for five days and test on the day following the last repetition, the average time between learning and test would be four days as compared

with one for the accumulated repetitions. On the other hand, if the test came after two weeks, the average time elapsed would be seventeen days for the distributed as compared with fourteen by the accumulated. The advantages of the distribution might be sufficient to overcome the smaller relative advantage of the long interval and not that of the short interval. There is nothing in the experiments to indicate why the difference should be more apparent with sense than with nonsense material. And the average time between repetition and test would be present for nonsense as well as for sense material. It is evident that other factors are necessary to complete the explanation. Whatever the reason, the increased effect of perseveration or the resulting 'setting' becomes more apparent after the lapse of considerable time.

## (2) Summary

(1) Divided repetitions, within limits, prove more effective than cumulative repetitions with logical material as well as with nonsense syllables. The greater value of distributed repetitions is particularly noticeable when material is tested two and four weeks after learning.

(2) For immediate recall, cumulative repetitions prove as effective as repetitions that are distributed.

(3) The forgetting of sense or logical material is rapid at first, then proceeds more slowly, as Ebbinghaus found for nonsense syllables.

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## IN AID OF INTROSPECTION

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Introspection is neither an esoteric art which can be practised only by the initiated, nor an instinct placed by Nature in the breasts of all in order that the study of psychology might be possible. It is a scientific method. In spite of the inroads of Behaviorism, most of us still teach that introspection is a very important psychological method. How many of us accept the implications of our position and *teach* that method? Text books on laboratory psychology give elaborate instructions designed to help the experimenter to the necessary technique, while it seems to be assumed that the much more difficult art of the observer will be "picked up." Now everyone can introspect, just as everyone can observe birds or flowers or any other natural phenomenon. But *scientific* observation implies a technique. This article is an effort to state the outlines of this technique in a form understandable to students. While I shall make no effort to conceal my own psychological articles of faith so far as these have been formulated, I shall attempt to state the method of introspection in a way which will be acceptable to those who do not have the same point of view in general psychology. At the same time, I shall offer a view of introspection which will not be acceptable to many. This view of introspection (as distinct from my general psychological position) cannot be separated from my account of the technique of introspection. One further caution: much that appears here will seem hackneyed or obvious to the professional psychologist. It has not seemed hackneyed to my students, nor am I aware of any account of the technique of introspection addressed to such readers.<sup>1</sup>

Introspection is a natural type of behavior. We all introspect frequently, just as we observe in other ways. For introspection is simply observing our own experiences. When we say "I feel warm," we have introspected. This may

<sup>1</sup> My obligations to G. E. Mueller and E. B. Titchener will be obvious to all who have followed the controversy aroused by Külpe's pupils. I owe much also to an unpublished laboratory manual by the late J. W. Baird.

or may not be scientific introspection, that is, observation which has complied with certain definite canons which make for accuracy and completeness.

The first difficulty is one of terminology. The words of daily life are woefully inadequate to scientific description. The first task of psychology is to refine terminology; the student's first task is to improve his power of distinguishing, classifying, describing, mental facts.

No real progress is possible in this task unless the student actually observes repeatedly the things described. The more you observe, the better you describe. The better you describe, the better you can observe and the more you will observe. This requirement is not peculiar to psychology. Compare the description of a flower made by the naturalist and the novice. Not merely is the first description clearer and more accurate in words; it is incomparably more complete in the details noted. To the beginner in any science, it seems discouragingly as if it were all a matter of "Words, words, words!" It is regrettable that we must spend so much time in the mere refinement of definition, but it is inevitable, not only for clear thought but also for complete observation.

Our next difficulty is that in psychology we do not observe things, but events or processes or happenings, the varying reactions of a mind or self to its environment. The thing you observe will not stand still while you watch. Indeed your watching is apt to make it change more rapidly; though one may add parenthetically that this last statement is by no means so universally true as would be implied from the majority of textbook discussions. The great trouble is not so much that introspection inhibits strong emotion, as that strong emotions tend to inhibit the introspective attitude. But the changing object of psychology is no peculiar difficulty. In many or most sciences, the object observed suffers changes rapid or slow. It is not inherently impossible to observe changing objects; it is merely more difficult. Laboratory devices may cause the object to change more slowly; increasing practice enables one to observe more and more of the swiftly moving panorama. One reason why students are introduced first to sensations rather than to affections is that they are relatively more stable, less swiftly moving. Finally we may take, as it were, moving pictures of the happening; each view is motionless, yet when taken in connection with others it closely simulates the real eventfulness of the experience. We may then proceed to examine a single view or snap-shot, to name the constituent features, to study their pattern or ar-

rangement. We may study the progress of these features from view to view. We must beware, of course, of thinking of mind as essentially thus a sort of kaleidoscope. The kaleidoscope is merely the way in which our minds represent and describe what is essentially a changing process.

It is further a unitary and not-divisible process, but this is not to say it is an unanalyzable one. I quote an illustration from Titchener;<sup>2</sup> a half-trained student reports in an experiment a feeling of "perplexity." Now perplexity is clearly a complex experience. A group of processes is present, some of which we can experience in other contexts, disjoined from each other. True, I have a fair idea of what he has experienced. But only a *fair* idea. The description should be so full and complete that one can imaginatively or sympathetically reconstruct the experience. Obviously, to say that you have "a feeling of perplexity" leaves much to the acuteness of one's imagination. Our first rule, therefore, is: *As far as possible, describe the constituent features of the experience in terms that resist further analysis. Describe in terms of part-processes which cannot be thought of as being themselves made up of smaller or simpler part-processes, or of part-processes found in other contexts.*

Is our task ended when we have completely described the momentary state of our experience in elementary or unanalyzable terms? By no means. Even Titchener, with his insistence upon the necessity of description as complete as possible in elementary terms, laments the current neglect of the durative or temporal aspect of experience. We must study the currents and eddies in the "stream of consciousness." Philosophers are not agreed whether we can directly observe change or can observe only sequence. The result is the same for our immediate purposes of psychological description and explanation. What we want is not merely the complete analysis of the isolated moment, but the movement of our experiences in time.

For psychology is not a purely descriptive science. We are not interested in description for its own sake. Psychology must help us to understand our complex selves, must supply the theoretic knowledge necessary to enable us to control our own behavior and that of others. To this end a description of the experience of a single moment, of a momentary snapshot of our experience, couched in elementary terms, is far

<sup>2</sup> E. B. Titchener, Description vs. Statement of Meaning, *Am. J. Psychol.*, xxiii, 1912, 167.

more useful than the average student is apt to realize. But it is not enough.

In the passage from which we quoted our illustration about perplexity, Titchener says that "the word 'puzzle' or 'perplexity' gives him the key to the observer's predicament, enables him to handle it, . . . but the word tells him nothing whatsoever of the observer's individual experience, of the particular 'feels' that constituted the perplexity in the particular case." Agreed! And we have just been arguing that the "particular feels" should be reported whenever possible. And yet if Titchener is right in saying that such an appellation as "perplexity" gives us "the key to the observer's predicament," do we greatly need the detailed analytic description? We do not want this description for its own sake, but precisely because it does give us "the import of the situation . . . enabling one to handle it." My criticism is that Titchener is too generous to the sort of report he is criticising. Such descriptive appellation is too crude a "key to the predicament." Analytic description would in most cases be much better. Yet just in so far as descriptive appellation does give us the "import of the situation" and enable us to handle it, it is very welcome.<sup>3</sup> I therefore add a second rule: *In addition to analytic description, experiences which are rapidly changing should be characterized or communicated by descriptive appellations, laying stress upon the sequences and order of the part-processes.*

I think it is because this kind of observation has been—not omitted, for it abounds in every piece of introspection, but rather—scorned and rejected of psychologists, that we have the weakness of experimental psychology on the dynamic side, on the side that considers time as a real factor in mental life. An example will show my meaning. In a certain investigation of mine, one of the subjects was thoroughly impregnated with the doctrine that introspection must be exclusively in terms of elementary experiences. Page after page of very excellent introspective analysis of his consciousness did he give me. But the whole thing was somehow dead; it did not *move*. In his desire to get all the processes analyzed into their elements, he was precluded by a sort of negative abstraction from attention to and report on the growth and move-

<sup>3</sup> It is possible that I have misinterpreted Titchener's meaning. The "predicament" of which he speaks may be wholly a laboratory predicament, the "key" merely a laboratory key. In any case I have to thank him for expressions so appropriate to the advantages of descriptive appellation.



ment of his experience. Only occasionally a statement about sequences would break through as it were surreptitiously. Analytic description is long and tedious. Be it never forgotten that it is invaluable; but a descriptive appellation can often give one a whole experience or some important part of it in a nutshell. And because it is so much briefer, it enables us so to observe the sequences that in the end we may get just that clue to the observer's predicament which is the common aim of both analytic and appellative description.

The danger of appellation is that we are tempted to infer analytic description from it. This we cannot safely do. These are parallel methods for arriving at explanation. Only where an appellation has been agreed upon as representing a certain analysis already made, may one justifiably draw any conclusions about "the particular feels in the particular case."

A further danger of characterization or appellation is the likelihood that it will be confused with interpretation. In my opinion, this is an overrated danger, but it exists. It is clearly illegitimate in your report of an experience to add one jot or tittle more than was in that experience. If, then, in your report, you add your interpretation of your experience, you have strayed from the truth. An example will suffice to show the nature of the error. In reporting the experiences incident upon rearranging a number of letters to make a word, an observer reported that "I shied off from that mass of consonants at the beginning, which could not possibly make a word." At best this is ambiguous. Did *O* mean that he framed more or less definitely the idea that such a mass of consonants could not make a word and that attention then shifted voluntarily to the end of the group? Cross-questioning brought out the true state of affairs: there was an easy non-voluntary shift of attention, following a slightly unpleasant perception of the many consonants. The whole clause "which could not possibly make a word" was an explanation after the fact. However true as explanation, such an interpretation is profoundly misleading if presented as a part of the consciousness of the given moment. Rule 3 therefore runs: *Include interpretation sparingly and always label it carefully as such.*<sup>4</sup>

As long ago as the 17th Century, Descartes called attention to the errors which slip into introspection, itself highly re-

<sup>4</sup> The above account of interpretation differs materially from Titchener's. I confess I do not understand what Titchener and his pupils mean by the term, but they clearly include what I have illustrated above and more.

liable, because of a persistent tendency to shift unconsciously from attention to experiences into attention to external objects.<sup>5</sup> As Titchener says, the error is both insidious and persistent. Introspection being defined as observation of one's individual experiences, everyone admits the impropriety of attempting to estimate the stimulus, which is an external thing. If introspection were to attempt judgments about the stimulus, its inadequacy would justify all the strictures that have been heaped upon it. How pitifully this pseudo-introspection falls short of accuracy, for example, in estimating the distance apart of two points of the esthesiometer! But that is not its proper task. What has introspection to do with physical measurements? Its task is to report on consciousness. In strict accuracy one ought to say, not "one point" or "two points," but "one pressure" or "two." No harm is done if it be clearly understood that "one point" as a report shall mean that the observer feels one clearly defined pressure. To insist otherwise would be to split hairs. Yet be sure you observe your convention. To slip over gradually into reporting "two points" whenever you *judge* that there were two stimuli leads to confusion. For such a report is not an unambiguous statement about your consciousness. You may have been aware of one pressure too large (so you judge) to have proceeded from one point. Yet according to your agreement with the experimenter, "two points" was to mean two distinct pressures; and this was not the case. On the face of the matter, the distinction seems trifling, but strict truth is never trifling. As a matter of fact, the failure to observe the distinction in this very experiment led to some very significant errors, and careful discrimination to their explanation. Rule 4: *Avoiding the "stimulus error," make no attempt to estimate the stimulus; confine your report to your consciousness, to your experiences. Nothing else is introspection; it is merely physical observation under difficulties.*

Rule 5: *Ordinarily describe experiences in their temporal order. But sacrifice this if necessary to catch some fleeting and elusive experience.* The advantages of this rule from the mere standpoint of convenience are too obvious to need comment. Its observance will also assist towards completeness.

Rule 6: *The experience or part of an experience selected for observation should not be too long, only a few seconds at the most.*

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<sup>5</sup> In his *Meditations*. See especially Meditation II, Everyman Ed., *Discourse on Method*, etc., 90.

This rule requires one or two comments. It is impossible for even the most practised observer to remember with sufficient accuracy and completeness the very complex happenings of a longer period than a few seconds. But many of the experiences which we desire to have described and explained take more time to their occurrence. If we stop the experience in mid course and commence to report on the part that has just taken place, we have no adequate guarantee that the experience would have gone on to the end that we suppose. If we allow the experience to terminate naturally, the observer is apt to forget the temporal course, to omit elements, and even to insert factors that were not present. One partial solution is to emphasize descriptive appellation, but as we have seen, this is often far from adequate. No single solution is possible. The writer believes that actual interruption of an experience should be resorted to with great caution. In all such cases, the experience should *previously* have been allowed to run its natural course. In most cases a satisfactory compromise may be reached by selecting certain portions of the experience for particular attention and report, while relatively neglecting the rest. If this be repeated often enough to allow of particular attention to each temporal part of the experience, an accurate picture of the whole can be obtained.

Rule 7: *Avoid "putative recollection."* This is a very common error among beginners and is frequently committed by those who should know better. It is the sort of alleged observation which begins with "Well, I guess I must have had" a visual image, or what not. Logical inference has its place in psychology. Less primary than introspection, it is possible that it is more important. But logical inference has no place in observation. It is fatal to observation. Of course guessing is much easier than observing, but science is not to be founded on such a basis. It is no disgrace to reply to the experimenter's question with "I don't know." But nothing so openly betrays your ignorance and inexperience as the attempt to conceal it by guessing.

So much for a few hints on the "How?" of introspection. But the "What?" has been treated as if it were entirely obvious. Of course this is far from being the case. It is likely that the student will be first set to observing experiences predominantly sensational. What are you to say about these experiences? First, you must be prepared to make a comparative statement about their attributes. Secondly, you must be prepared to state the patterns of combination into

which the elementary experiences enter. Now psychologists are not agreed as to just what attributes we can assign to the various kinds of sensations. As observers, therefore, we must keep an open mind. We must have in mind all the attributes which any competent person has alleged to belong to the sensation and see if we can find them. There are seven candidates which are commonly urged for the office of sensational attributes. These are quality, intensity, extensity, durance, local sign or order, temporal order, clearness. Of these the first two are obvious in every sensation. No one questions that every sensation has some particular quality. Your task in observing a relatively simple experience is to *identify* the several qualities which compose it. Intensity is another attribute which is obvious to our untutored observation.<sup>6</sup> Here our problem is more difficult than with quality. Intensity is an attribute which carries from just more than zero to a maximum, while quality is strictly speaking invariable.<sup>7</sup> Hence we must make some sort of comparative statement about the intensity; we must measure it in terms of some scale of intensities. This was once deemed strictly impossible, though we all make such reports whenever we compare the loudness of two sounds. The possibility of accuracy in such determinations is another matter and one which depends upon the development of experimental methods, among others upon the avoidance by the *O* of the stimulus error referred to above.

As implied above, quality and intensity are very simple attributes and are open to very unpractised introspection. This is not always the case with the others. Thus local sign, while easily identified in the field of touch, and without great difficulty in vision, as the not-further-analyzable factor which differentiates one touch from another, or one tiny speck from another precisely like it imaged on another part of the retina, —while easily identified in these cases, is with great difficulty to be found in others, if at all. For if it is found in other senses, it is in Watt's phrase "wrapt up in the complications and modifications which experiences produce upon one another." To discover an attribute thus wrapt up, familiarity with that attribute in other senses where it is more obvious is implied, and may even then be very difficult. Not infre-

<sup>6</sup> Compare the illuminating discussion by H. J. Watt in the Introduction to *The Psychology of Sound*.

<sup>7</sup> This is to say, when the quality changes, we have a different sensation. This makes quality in some sense fundamental to the other attributes, and so it is, as men have recognized in naming sensations from the qualities.

quently we are obliged to state the derived, complex attribute which we find in the experience (such as its localization, which is derived, it is now commonly believed, from local sign and extensity in combination). Nevertheless we must in each case question our sensational experience for the presence of each of the seven attributes given above. Extensity or volume, the primitive "bigness" of a sensation, and durance or the primitive "lastingness" are, like intensity, attributes which vary from just more than zero to a maximum (whether within the range of one sensation or not appears to be an open question) and hence our problem is to measure them. In actual practice we are accustomed to measure the derived attributes based on these, extension and duration, and students sometimes find some little difficulty in limiting themselves to the simple attributes.\* Local sign or order, and temporal order resemble qualities in that they do not vary from zero to a maximum. Yet these attributes, unlike quality, are quantitative, since they can be summed. Temporal order and durance are said by many psychologists to be derived, dependent upon the presence in consciousness of more than one sensation, and their uniform and necessary presence is denied. We may therefore question our experience as to whether these characteristics are present and if present as to whether they are simple.

Clearness is a characteristic of sensations whose systematic position is doubtful. These doubts need not concern us as observers. Every sensation has some degree of clearness and as introspectors it is our duty to determine how great this is.

There is yet another characteristic of many sensations, which is denied an attributive rank by most systematizers, but which should be reported on by the introspector in much the same way as the attributes. I refer to the hedonic tone or the amount of pleasantness or unpleasantness of the sensation. This point need not be labored, for it is the writer's experience that unpractised introspectors devote most of their energy to report on this factor.

We have then eight points on which to question our sensory experiences: quality, intensity, extensity, durance, local sign or order, temporal order, clearness, and feeling tone. At times, as was said above, the student will be unable to find these lying open to introspection, but will be obliged to report the presence of such derived attributes as show their presence indirectly. The two orders in particular are almost

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\* The student will probably find most help in establishing the distinction in W. James. *Principles of Psychology*, II, 1890, 135.



invariably found as spatial and temporal localization, derived attributes depending upon the co-operation of two or more experiences.

However satisfactory this may be for the systematizing psychologist in proving the presence of the primitive attribute, it is unsatisfactory from the standpoint of the second duty laid upon the observer of sensational experiences. Not only must we report upon the attributes of the sensations present in a given experience, but we must report the pattern or scheme of combination of these sensations. We want to know precisely what primitive attributes go to make up these more complex attributes. What besides extensity does enable us to localize? What aspects of an experience take a dominating place in consciousness? "A recent writer has declared that the 'texture' of qualitative perception, due to the 'massing' of its sensory elements,—it is difficult to find words to indicate precisely what is meant,—may, in certain spheres, be as important in creating apparent qualitative differences as is the quality of the single sensation."<sup>9</sup> Unfortunately little can be said to help the student in his effort to describe the pattern of these complexes beyond a renewed insistence upon the value of analysis, not as the end but as the beginning of this process.

It is time to give the student some encouragement. The number of things that seem to be required of him are many and the difficulties not slight. Were it not for one factor, the accomplishment of our task would be almost impossible. The intention to introspect, to attend to one's experience, materially facilitates its own task. Our powers of observing are heightened by direction of attention, especially with increasing practice and familiarity with the phenomena of the general kind to be observed. But in addition to heightening our power of observation, we get a sort of placing or classifying of the new experience in terms of familiar psychological rubrics or concepts. Sometimes the experience will be explicitly placed by means of a sentence in internal speech, sometimes there will be only a fleeting verbal gloss by means of which the new experience is assimilated to old and familiar classes. Often enough the chief instrument in such apperceptive classification (as it is called) is the name of the group to which the experience belongs. Thus Stricker writes: "When, after my experimental work, I proceed to its description, as a rule I reproduce in the first instance only words

<sup>9</sup> E. B. Titchener, *A Text Book of Psychology*, 349, quoting E. Murray, Organic Sensation, in *Am. J. Psychol.*, xx, 1909, 446.

which I had already associated with the perception of the various details of the observation whilst the latter was going on. For speech plays in all my observing so important a part that I ordinarily clothe phenomena in words as fast as I observe them."<sup>10</sup>

Apperception may take place in non-verbal ways, though perhaps introspective apperception is more effective if in words. Sometimes the experience is apperceived while it is running its course; more often, with the unpractised subject, it takes place immediately afterward.

Closely allied to this is a phenomenon which is probably most familiar in the field of hearing. You say something "back-end-to" and people laugh. Suddenly you realize with startling vividness what you have just said. It is not a peripheral thing like the after-images of vision and touch, which are more properly called after-sensations, yet it is more like these than ordinary memory or fancy images. Now under the influence of the intent to observe, a great proportion of our experience can be preserved in a memory after-image of this sort. Under experimental conditions it is highly reliable and greatly facilitates that completeness of report which would otherwise be so difficult.

It has been assumed in the foregoing that the reader is familiar with the general conditions necessary to any observation; care, accuracy, honesty, etc. The discerning cannot have failed to notice, however, the constant stress laid upon practice and the intention to observe the psychological facts. Indeed this intentional direction of attention, and the implied familiarity with the general nature of the phenomena to be observed, are the secret of valuable introspection. I began by saying that anyone can introspect. I conclude by urging that the value of one's introspection is almost directly proportionate to one's habitude. Anyone with a good memory and a sincere desire to improve can learn to introspect in a way which will be of distinct scientific usefulness.

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<sup>10</sup> Quoted by Wm. James in *Psychology, Briefer Course*, 1892, 309.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

XLIX. AN EXPERIMENTAL STUDY OF CUTANEOUS IMAGERY

By CATHERINE BRADDOCK

Very little experimental work has been done upon cutaneous imagery, and that little has no more than a preliminary value. The present study was undertaken to find out (a) if cutaneous images of pressure, warmth and cold occur, and in that event (b) if the images are free or tied, that is, if they are capable of sustaining by themselves alone the whole meaning of a past perception, or if they appear only in the train of other images (visual, auditory, kinaesthetic) which have proved to be thus capable. The outcome is so surprisingly negative that it, too must be regarded as only provisional. We have some good observers, but only a rough method.

*Experiments: Series I*

Our object in a first series of experiments was to obtain a direct comparison of sensory with its corresponding imaginal experience. The sensory stimuli were areal (2 by 1 cm.); metal blocks, rounded at the corners, for warmth and cold; wood covered by felt for pressure. We used two markedly different degrees (weak and strong) of the three qualities. For weak cold the metal block was cooled in water of 10° C, for strong cold in ice-water; for weak warmth the stimulus was heated in water of 4° C, and for strong warmth in water of 10° C, above the temperature of the skin. The cutaneous surfaces investigated were those of forearm, upper arm, forehead and cheek. O sat with closed eyes, and the stimulus was set down for 1 sec. Chance determined the order in which the six stimuli should be given. The instruction, in the sensory series, ran: "I shall stimulate the skin [of your forearm, etc.]. When the stimulus is removed, describe the cutaneous experience as well as you can." We thought that the reports of this sensory series might help in O's training, as showing him what to look for in the image; we thought also that, if there should be differences between image and sensation, the differences would stand out the more clearly for the immediately previous experience of sensation. We realised that our heavy pressure stimulated deeper-lying organs, and that our temperature stimuli aroused pressure as well as cold and warmth. It seemed best, in a first attempt, to take these complications into the bargain.

At the conclusion of the sensory series on a given surface there was a pause of 5 min., and an imaginal series began. The instruction ran: "I want you now to think of one of the sensory experiences of the last series. When I have named it, realise it as fully as you can, and then describe it. Think of [strong cold, etc.]." Chance again determined the order.

In general it was found unnecessary to present a sensory series for the various surfaces more than once. Three imaginal series were performed for the four surfaces, so that there were 12 series in all. As these continued two pressures, two warm pressures, and two cold pressures there were possible 72 pressure-images, 24 cold-images and 24 warmth-images.

### *Series II*

In a second series we asked *O* to realise familiar situations in which an image of pressure, cold or warmth might be expected to appear. It seemed possible that, under these conditions, images might be aroused with greater ease. The instruction ran: "Realise the following situation imaginally, and describe it as well as you can, paying particular attention to [cutaneous pressure, etc.]. Think of yourself as"—and then a situation would be given. There were ten situations for the three kinds of experience. We used for cold: stepping into a cold bath, having a snowflake fall on the face, holding a snowball in the hand, feeling a key slip down the back, stepping out of a warm house on a frosty day, laying a finger on a cold radiator, taking too large a spoonful of ice cream, waking up at night to find the bedclothes slipped down, having an icepack on the forehead, feeling a dog's nose against the palm. For warmth: bending over an open fire, stepping into a hot bath, putting the hand in the oven, taking a pot off the range, sipping warm cocoa, stirring a pan of boiling sugar, laying the finger on a warm radiator, clapping the hand round an electric bulb, touching a hot-water bottle with the foot, having a pad of cotton wool bound over the back of the hand. For pressure we tried to find situations which should, so far as possible, rule out kinaesthesia: the feeling of a leaf falling on the back of the hand, the feel of fur against the skin, the feel of an insect on the neck, lying on a stone, the touch of a hand on the shoulder, the feel of the wind beating on the face on a gusty day, a hair-shampoo, being brushed down by a porter, receiving a handful of change from a conductor, the flutter of a moth on the arm.

### *Observers*

There were five *O*'s: Dr. L. B. Hoisington (H), instructor in psychology; Dr. H. G. Bishop (B), assistant in psychology; Mrs. A. K. Whitchurch (W) and Miss C. Braddock (Br), graduate students; and X, an untrained *O*. H and B had recently had extended training in the observation of kinaesthetic imagery.

### GENERAL RESULTS

(1) We cannot say positively, in the light of our results, whether images of cutaneous pressure, warmth and cold occur. If they exist, their appearance seems at best to be rare; and they are so interwoven with sensory experiences (changes in temperature and tension of the skin, kinaesthesia) and with visual and other images that they are very difficult to detect. The *O*'s differed greatly in their reports. W and B, in the majority of cases, were unable to obtain anything but very doubtful images; X, on the other hand, reported their occurrence readily. H and Br were also doubtful in a large proportion of their reports, but obtained at times what seemed like clear cases of



imagery. There may very well be individual differences. But there is no doubt that, in the experience of realising imaginally the former perception of stimulus or situation, meaning may be mistaken for process. Time and again H (our most practised *O*) would report that a former experience of pressure, warmth or cold had been adequately realised, and yet that he could not say whether an imaginal process in kind, a quality of pressure, warmth or cold, was present in the imaginal complex. H also frequently reported a tendency to say that he had obtained an image; he might even set himself to describe it; and then he would realise that some other image had carried the meaning of the cutaneous experience, and had thus suggested that the required qualitative image must be there. Br reported the same sort of thing.

W's and B's doubtful cases were largely of a different order. Many of them were reported as probably sensory and not imaginal, the sort of pressures and temperatures sensed when one attends keenly to a given part of the skin. Both *O*'s, however, and W in particular, reported cases like those of H and Br.<sup>1</sup>

(2) If cutaneous images occur, there is little doubt that they are tied and not free. Practically all the cases in which images appeared as free are cases in which the *O* reported a doubt whether his cutaneous experience was sensory or imaginal. Those cases, on the other hand, which approached nearest to true imagery (though we cannot vouch certainly even for them) were invariably tied either to visual or to kinaesthetic imagery.

#### RESULTS IN DETAIL

*Pressure.*—The reports of the *O*'s differ very greatly. H was more sure of his pressure-images than of the other two kinds. He and X found the images easy to get, and rarely failed to report them. W and B, on the other hand, were in most instances doubtful. Br in Series I regularly reported pressure-images, but became more and more doubtful of them in Series II; deep pressures (kinaesthetic) seemed to be involved. W and B often reported kinaesthesia, which appeared to serve as a substitute for cutaneous pressure.

All of H's reports in Series I contained pressure-images, many of them more than one. The reports began typically with a pressure-image, limited in extent and of brief duration, followed by visual imagery ("bit of lighter grey against a darker grey background") and eye-kinaesthesia, which together served to localise the pressure at the required spot upon the skin. "An experience realised in pressure-terms alone," H remarked at the end of the series, "is a generalised kind of thing, without concrete setting, whereas when the visual image is present it makes the experience definite and particular." The

<sup>1</sup> It is plain that results like those of G. H. Betts (*The Distribution and Functions of Mental Imagery*, 1909, 45 f.) must be taken with many grains of salt. Nor is it of any use to improve one's questionnaire; the fault lies, not with the questionnaire, but with those who are called upon to answer its questions. X, our untrained *O*, gives consistently positive results (in full agreement with those of Betts), and yet it is certain that, whether or not imaginal processes were present, he was reporting meanings and not processes. We must in fairness add that much water has flowed under the bridges since 1909, and that Betts would hardly write to-day as confidently as he wrote a dozen years ago.



pressure-image usually recurred after the localising and particularising had taken place. Sometimes visual imagery or eye-kinaesthesia or auditory-motor processes might occur before the pressure-image appeared, and sometimes the visual and pressure images alternated several times before H was satisfied that his realisation was adequate.

Here, then, there seems to be a clear-cut case of tied pressure-imagery, with visual imagery and eye-kinaesthesia serving as support. We noticed, however, at the beginning of Series II, that H did not (apparently) distinguish between pressure and kinaesthetic imagery; pressure was often reported as "meaning movement."<sup>2</sup> Since the other O's frequently reported kinaesthesia as such, we thought the point worthy of further experiment. We therefore performed short sensory and imaginal series on subcutaneous pressures (etherised skin) and the sensations involved in raising the arm. In the imaginal series the deep pressures became 'surfacy,' bidimensional, though they were not necessarily localised at the surface of the skin. The results appear to show that, at any rate under these rough conditions, there is for H no difference, in process-terms, between subcutaneous and cutaneous pressure.

If this interpretation is correct, H's reports square very well with those of Br. Here the typical report is that of a visual image representing the stimulus, followed by a deep pressure, referred by the O to kinaesthesia. In the effort after full realisation of the perceptive experience these images alternated, the pressure coming nearer and nearer to the surface, until finally it appeared as a flash-like process on the skin itself. The visual image of the stimulus was necessary to O's realisation of the corresponding perception. The flash-like pressures were (as has been said) at first accepted as cutaneous; later on the O became very doubtful of their validity.

B, on the other hand, reported typically pressures of large area and long duration, which seemed to be sensory in nature, and to resemble the pressures brought out by sustained attention to the skin. It is possible, however, that kinaesthetic imagery was involved; since, after a supplementary series on deep-seated pressures (similar to that taken with H), B remarked that all the pressures of Series I and II had extended a little below the surface of the skin. Kinaesthesia itself was often reported. B was reluctant, throughout the experiment, to commit himself to an imaginal report.<sup>3</sup>

W relied for realisation of her perceptive experiences mainly on visual imagery. She also reported a great deal of kinaesthesia. Her usual 'cutaneous images' were of the same sensory nature as B's. She remarked in both series that if there were no visual image there would certainly be no cutaneous; and in Series II she reported no cutaneous image that was not possibly sensory.

We thus have three possibilities as regards the image of cutaneous pressure.

(1) True images exist, but occur only as bound up with visual or kinaesthetic images. Evidence for this view may be found in the reports of H and X (visual support), and in many of the reports

<sup>2</sup> In the situation of stepping into a cold bath, movement in the leg and shiver in the back were gross enough to be noticed by E, although H reported pressure only.

<sup>3</sup> We have tried to tabulate the reports, but have found the task impossible. The O's 'hedge' and qualify from the start, and grow more and more doubtful as the experiment proceeds.

of Br (kinaesthetic support), and in a few reports of W (visual support).

(2) Deep-pressure images are mistaken for images of cutaneous pressure. This view accords best with Br's results, and in part with those of B. The meanings of skin-surface, localisation, and extension, are carried in visual terms.

(3) Sensory kinaesthesia is mistaken for imaginal cutaneous pressure, and together with visual imagery carries the meaning of skin-surface, localisation, extension. Evidence for this view may be found in the reports of X, W and B; to some extent also in those of Br.

It is possible to read H's results in the sense of (2) and (3), especially since we know that H was set to report qualities, and (as he told us later) avoided the term 'kinaesthetic' owing to its merely functional meaning. As Br and W became increasingly doubtful with practice, and as X's reports may be discounted, the evidence for (1) is very slender. We have no doubt, on the other hand, that (2) and (3) cover part of the facts; the only question is whether they are adequate to the whole.

*Warmth and Cold.*—All the O's except X were uncertain of these images; every report has some measure of doubt attaching to it. H and W were less sure of cold than of warmth, Br was less sure of warmth than of cold. The reports fall into four more or less distinct groups: (a) a deep intense warmth or cold with something pricking or stinging about it; (b) a diffuse experience, like a breath of warm or cold air sweeping over a largeish area of the skin (fair intensity, long duration), which runs into sensory warmths or colds elsewhere in the body; (c) an experience like a puff of warm or cold air, small in extent, very weak in intensity, short in duration; and (d) an experience of pressure, with warmth or cold inherent in it. In the case of warmth there was a variant of (b), in which the warmth began in a small area and spread gradually in waves till it merged with the bodily warmth elsewhere.

The experience reported in (a) occurs but rarely (H, Br, W). It seems to be kinaesthetic in nature. Experience (b) is often reported by all O's, and is typical for B. It seems to be obviously sensory; indeed, the qualification "perhaps sensory" is appended to many of the reports. The variant is also probably sensory, at least in some measure. It occurred typically in X's reports, always bound up with visual or pressure imagery.

There remain (c) and (d), in which if anywhere we shall find true images of warmth and cold. The experiences of (c) were extremely vague and elusive, "just a flash and then gone," so that the O's found description almost impossible. Since the stimuli of Series I combined temperature with pressure, there was greater variety under (d): pressure might appear without warmth or cold, pressure might immediately precede or follow warmth or cold, pressure and warmth or cold might occur together, at roughly the same intensity and clearness, or pressure might be predominant, with a tinge of warmth or cold added to it. H and Br report all these modes of experience, though with the recurring doubt of the imaginal character of warmth and cold; W and B were too much in doubt to do more than report a possible temperature as given with the visual image or the pressure.

Visual images were important for all O's except B, and were indispensable to W. In the latter part of Series I, H and Br tended to drop vision for pressure (kinaesthesia?), but the visual images were

prominent again in Series II. If temperature images occurred, they were certainly tied, either to vision or to pressure. We did not experiment with radiant heat or cold.

*Other Factors.*—In both the sensory and the imaginal series H and W frequently reported the kinaesthesia of eye-movement, accompanied or followed by visual imagery. The report was less common with B and Br.

Auditory-motor (verbal) processes played an important part in H's reports. They often appeared at the beginning of his train of imagery, and carried a generalised reference to the previous experience. In the temperature series they might further carry the particular meaning of cold or warmth, and thus render it doubtful whether an imaginal temperature was experienced. They also carried the meaning of confirmation, acceptance or denial of the adequacy of a given process-complex as realisation of the former experience.

B relied largely upon auditory-motor processes; they might so fully mean the past experience that no other processes of any kind were reportable. In Series II B introduced auditory-motor images into the situations, in order by their aid to make his task easier. He reports more adequate realisations in this Series than in Series I, though his cutaneous imagery does not become more assured.

The other O's have little to say of auditory-motor processes.

*The Methods.*—The method of Series I is a variant of that recommended by J. R. Angell.<sup>4</sup> Preliminary work with blunt points had led to very confused and unsatisfactory results; we therefore enlarged the area of stimulation, in order to give form and substance to the perception and a better foothold to the image. The method of Series II was suggested by the work of A. H. Sullivan on kinaesthetic imagery.<sup>5</sup> We have no doubt, in view of the competence of our four principal O's, that either method would have detected the presence of free cutaneous imagery. None of these O's, in fact, complained of the methods; recall and realisation were feasible enough; the difficulty lay in making sure of the image. Advance thus seems to depend rather on a sharpening of O's *Einstellung* than on improvement of technique.

#### CONCLUSIONS

A conservative reading of our results leads to the following conclusion. Where O's imaginal type tends to be one-sided (B. W), the imagery ordinarily employed comes up so readily and so dominantly that any possible traces of cutaneous imagery are obliterated; the O's report substitutive imagery only. Where the type is generalised (Br, H), cutaneous imagery of pressure, warmth and cold occurs rarely in subordination to free imagery of vision or kinaesthesia.

This is the conclusion that Br and H themselves incline to accept. Both O's doubt the occurrence of cutaneous imagery in the situations of everyday life; both are disposed to think that under the conditions of the experiment they sometimes obtained it.

There is, however, no proof of this position; and there is full proof of the trap set by meaning for the untrained or momentarily unwary observer.

<sup>4</sup> Report of the Committee of the American Psychological Association on the Standardising of Procedure in Experimental Tests, 1910, 94 f.

<sup>5</sup> This JOURNAL, xxxii., 1921, 54 ff.

## L. THE INTEGRATION OF PUNCTIFORM COLD AND PRESSURE

By S. TUNG

The experiments described in this paper were undertaken to discover what psychologically results from the simultaneous stimulation of cold and pressure spots. So far as we are aware no previous attempt has been made to synthesize cold and pressure with punctiform stimulation. Some investigators, however, have said that the real stimulation of the organs of these two qualities gives the experience of wetness. Helmholtz, for instance, remarks that "das scheinbar einfache Gefühl des Nassen, welches ein berührter Körper erzeugt, . . . aus dem des Glatten und des Kalten zusammengesetzt [ist],"<sup>1</sup> and Bentley has shown that under experimental conditions wetness may in fact result from stimulation with a dry smooth object (such as rubber sheeting) the temperature of which is either above or below that of the skin.<sup>2</sup> Thunberg, on the other hand, writes: "Eine Kälteempfindung, welche von einer Berührungsempfindung nicht begleitet ist, wird häufig als von einer Flüssigkeit verursacht aufgefasst, was daraus erklärt werden kann, dass beim Niedertauchen eines Körperteiles in eine Flüssigkeit der Drucksinn fast gar nicht erregt wird, dass also die entstehenden Temperaturempfindungen ungemischt sind. Im Zusammenhang damit steht eine eigentümliche Illusion von Nässe, welche entsteht wenn man die Stirn mit einem sehr kalten Gegenstande . . . B. 20 Sek. berührt. Nach dessen Fortnahme dauert die Kälteempfindung noch einige Zeit fort und zugleich hat man den Eindruck, dass die Stirnhaut deutlich nass sei."<sup>3</sup> Fröbes says, even more emphatically: "Die Qualität 'nass' scheint nichts anders als einfache Kälteempfindung ohne gleichzeitige Berührungsempfindung."<sup>4</sup> The difference of opinion expressed by these authors can, of course, be settled only by experiment, and since we employed a punctiform and not an areal mode of stimulation, we are unable definitely to settle it. We may say, however, that under our conditions wetness (or, more accurately, 'cold wetness') was found frequently to occur with simultaneous stimulation of pressure and cold spots, and that the experience was never observed when spots adequate only to their normal stimulus were excited singly. We now give a more detailed account of our methods and a more complete statement of our results.

Our first method was to stimulate with a cold-point spots on the surface of the fore-arm which responded both to pressure and to

<sup>1</sup> H. Helmholtz, *Physiologische Optik*, 1866, 698; 1896, 845.

<sup>2</sup> M. Bentley, *The Synthetic Experiment*, this JOURNAL, xi, 1900, 414ff. See also E. B. Titchener, *Beginner's Psychology*, 1915, 47.

<sup>3</sup> T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, i, 1905, 708.

<sup>4</sup> J. Fröbes, *Lehrbuch der experimentellen Psychologie*, i, 1917, 134. No authority is cited for this statement, but we suspect that it was taken from Thunberg's article. Thunberg does not mention Bentley's paper.



cold. We had planned to check the results thus obtained by stimulating in the same way cold spots that did not respond also to pressure, but we were unable to find spots of this kind because the intensity of pressure furnished by our apparatus was too great. As stimulus we employed a thermaesthesiometer with cold water running through the point. This was attached to a spring balance modeled after Thunberg's algometer,<sup>5</sup> and the whole was mounted on a universal standard. The amount of pressure, as measured on a pressure balance, was 16 gr., with an mv. of 10% due to the difficulty of controlling adequately the weight of the water conduit. The temperature of the water varied between 18° and 21° C., and that of the room in which we worked between 21° and 26° C.<sup>6</sup> The point of the aesthesiometer was smoothly rounded and measured 1 mm. in diam. The fore-arm was placed in a warm plaster cast. The spots to be stimulated were permanently marked by tattooing. The observers were Professor Weld (W), Dr. L. B. Hoisington (H), Mr. S. Takaki (Ta), and the writer (Tu); Mr. Takaki served as *E* for the latter. The instruction given to the *O*'s was as follows: "I shall stimulate a spot on your arm, and you are asked to report the course, particularly with regard to quality and intensity, of the cutaneous experience."

*Results obtained by the First Method.* In early experiments the course of the experience was, in general, described by all *O*'s as, first, a pressure of low intensity which quickly passed into a cold; this, after an interval, changed into another cold which then, gradually and with fluctuations, faded out. Later the two qualities of cold were distinguished, the first as a superficial 'wet' cold, the second as 'just cold,' deep and penetrating. Typical characterizations of these colds are as follows:

W. "I felt chill, like a piece of ice touching the skin, which first penetrated deeply then quickly spread out." "Wet was felt but it disappeared rather quickly, then a deep cold that was not in the least wet." "I had first the impression of wetness like a drop of cold water."

H. "At intermediate intensity I felt something like a piece of ice with a strip of cloth surrounding it. Wetness was not sharply felt except at the edge of the spot; it did not last long. After wetness was gone, just cold was left behind." "The wetness was like water; as I felt it less as the cold became more intense." "The wetness was on the surface; the cold beneath the surface of the skin."

Ta. "I felt something smooth wet and cold at the spot like ice melting; . . . after wetness was gone I felt simply cold like the coldness of metal."

The wet cold was reported in about 80% of the trials; in some instances cold did not appear at all; in others it was long delayed, as a result, we suppose, of a relative displacement of the cold-organ and the tattooed spot. It was a logical inference that the change from the wet to the dry cold might be due to the earlier adaptation of the pressure, and we found by rough experiments that the average adaptation time of pressure was in fact only about  $\frac{1}{4}$  that of cold. If the above inference is correct, we should be able to obtain a return of the wet cold by restimulation of the spot with a pressure-stimulus

<sup>5</sup> T. Thunberg, *Skand. Archiv.*, xii, 1902, 424.

<sup>6</sup> A lower temperature of the water was avoided because of the production of moisture by condensation on the metal point.



after the wet cold has gone but before the second cold has disappeared. We tried this experiment, and in some instances were successful. It was essential to success to employ a relatively weak stimulus (a hair), to apply it as close to the cold stimulus as possible, and to direct *O*'s attention to the cold; if the cold was not focal, a pressure was reported within an area of cold.

Our second method was that of simultaneous stimulation of discrete pressure and cold spots. For pressure we employed hairs of 3.0 and 4.2 gr/mm tension value; for cold it was essential to our method to excite the cold spot without touching the skin. In order to do this we found by trial that exceptionally low temperatures of stimulus were necessary; and the only one that proved adequate was obtained from a mixture of ether and carbon-dioxide snow.<sup>7</sup> This was managed in the following way: a Willyoung thermaesthesiometer, with thermometer removed, was clamped to a universal standard; it was first filled with the snow, and then the ether was added. When this was done the temperature of the mixture fell quickly to about  $-78^{\circ}\text{C.}$ , a heavy vapor streamed down perpendicularly from the instrument, and a thick frost accumulated on it. Since the vapor was felt as a 'damp cold,' and falling particles of frost as 'wet cold,' we were forced to wait until the violent stage of evaporation was over; thereupon the stimulus was ready for use. Our subsequent procedure was as follows: the cold stimulus was first lowered and adjusted in such fashion that the point of the instrument was directly over a cold spot. The distance above the surface of the skin varied from 2 mm. to 10 mm., depending upon the state of the mixture or upon the readiness of response of the cold spot. When *O* had reported a cold of good intensity, a signal was given, and a pressure spot in the immediate neighborhood of the cold spot was stimulated by the hair. *O* was instructed to describe the qualitative course of the cold after the signal.

*Results obtained by the Second Method.* We again frequently, although not invariably, obtained reports of 'wet cold.' At times, however, the pressure was not realized either as pressure or as anything else, and at others pressure was felt in an area of cold. These results seemed to be due either to a lapse of attention to the cold (a condition which our instruction was designed to meet), or else to the lack of a proper proportion of the intensities of cold and pressure; for example, we found that the pressure was most frequently not felt when the intensity of cold was strong, and that it generally appeared in a spatial pattern with cold when the cold was weak. We regret that we were unable, for want of time, to make a systematic study of this point. But it appears probable that the appearance of 'wet cold' is, aside from the mode of stimulation, conditioned both upon the greater clearness of cold and upon a certain relation between the intensities of cold and pressure. When the 'wet cold' occurred it came sud-

<sup>7</sup> We tried without success the apparatus of the first method with the point cooled by an ice-salt solution, a temperature cylinder also cooled by an ice-salt solution, a mica shield with small holes in it for receiving the pressure stimulus and for conducting cold from a block of ice, and an ice-ammonium chloride solution in the Willyoung apparatus. The department of physics in Cornell University kindly supplied the carbon-dioxide snow, and gave us the temperature of the mixture.

denly, with surprising distinctness, and lasted for a very brief interval. It was characterised as like a "melting flake of snow," "a drop of cold water," "a tiny particle of ice." The 'radiant cold' on the other hand was reported as "not punctiform but areal, less intense, and more penetrating than the 'wet cold;'" "it was like the 'dry cold' of the other experiment," "like the feel of a cool breeze without pressure, or the cold of evaporating ether."

The fact that 'wet cold' may derive from a simultaneous stimulation of cold and pressure spots is, of course, only a single result. The duration of the experience was, under our conditions, so brief that our *O's* were baffled in their attempts to give a qualitative description of wetness. But an investigation of the synthesis of pressure and warmth with punctiform stimulation is now being made in the laboratory, and a systematic study of Thunberg's illusion is also under way. These two inquiries should throw further light upon that problem.

## LI. THE HERING COLOR-BLINDNESS APPARATUS AND THE NORMAL RAYLEIGH EQUATION

By M. WINFIELD and C. STRONG

In a previous article on the Hering Color-Blindness Apparatus reference was made to experiments then in progress on the normal Rayleigh equation, the matching of spectral red and green to yellow.<sup>1</sup> In this paper we report the results of those experiments. It was our purpose not only to test further the capabilities of the Hering instrument, but also to determine the range and form of distribution of the matches of a large number of *O*'s who have normal color vision. That there is a wide variation among normal *O*'s has, of course, been known since Rayleigh first performed his experiment,<sup>2</sup> but interest has centered upon the extremes of variation (the anomalous trichromats), and the central variations have for the most part been either ignored or else explained away as the result of 'errors of observation.'

*Conditions and Procedure of the Experiment.* The experiment was performed in a dark room, and the apparatus was illuminated by an artificial light of Gage's glass<sup>3</sup> as described by Cowdrick and Winfield.<sup>4</sup> The horizontal distance from the center of the roundel to the light chamber was 13 cm.; the vertical distance from the top of the table upon which the apparatus stood to the center of the roundel was 27 cm. It was found by test that a variation of from 2 to 12 volts of current below the 110 at our disposal did not affect the results of the experiment. *O*'s eyes were protected from the glare of the lamp by a screen.

In making the equation we place a green glass in the left (to one facing the open side of the instrument), a red glass in the floor, and a yellow glass in the right aperture of the light chamber. The red reflector is set at 20° and the yellow at 15° of arc;<sup>5</sup> the former is

<sup>1</sup> M. Cowdrick and M. Winfield, The Adjustment of the Hering Color-blindness Apparatus, this JOURNAL, xxx, 1919, 418.

<sup>2</sup> Rayleigh, Experiments on Colour, *Nature*, xxv, 1882, 64; On Defective Colour Vision, *Brit. Assoc. Rep.*, 1890, 728. For a review of subsequent experimentation see J. von Kries, in Nagel's *Handbuch d. Physiologie des Menschen*, 1905, 124ff., and in Helmholtz' *Physiologische Optik*, ii, 1911, 343ff.; J. H. Parsons, *Colour Vision*, 1915, 182ff.

<sup>3</sup> We have recently learned that not all roundels of Gage's glass have, as have those employed in this and former experiments in the Cornell laboratory, a ground surface. At the present time the manufacturers supply the glass as it comes from the press unless a ground surface is specified.

<sup>4</sup> *Op. cit.* They give incorrectly the horizontal distance from roundel to light chamber as 'about 30' cm.

<sup>5</sup> This setting of the red reflector was chosen because it permitted a green of excellent chroma at the one extreme of the variable. The 'mixing value' of R in this apparatus is much greater than that of G; e.g., if the red reflector had been set at 40° of arc all the green

not moved throughout the experiment, and the latter is moved only when, after the match in hue has been made, *O* reports a difference in tint. The green reflector which conditions the variable stimulus is set to give R or G as *E* desires. *O* is then instructed to make the equation by turning the knob at his right hand (which controls the green reflector) to the right or to the left, as the case may be, until the match is made. In these experiments ten successive matches were required of every *O*, and the setting of the green reflector for every match was made according to the schema Rl, Gs, Rm, Gl, Gs, Rm, Gm, Rl, Gl, Rs, the capital letters signifying a setting of the reflector that would give R or G, and the small letters long short and medium distances from the approximate degree of arc that would give the match. In order to avoid the effect of local adaptation (which is an important source of variation in a single set of matches) *O* after making a match was required to rest his eyes for a few moments and then to verify, and if necessary to rectify, the equation before the scale-reading was recorded.<sup>6</sup> In all matches *O* used but one, usually the right, eye.

*Results.* Our results fall, according to the aim of our investigation, into two groups: those which concern the capabilities of the Hering Color-blindness Apparatus, and those which indicate the range and distribution of the matches of normal *O*'s. As regards the first of these, we found early in our experimentation that the apparatus, when employed under the conditions we have described, gives homogeneous results; but that, owing to the mechanical adjustment which controls the movements of the reflector, the units of measurement are too large. The adjustment is so 'coarse' that a change in the position of the reflector by less than a degree of arc, an amount that frequently destroys a match, is not easily made. The effect of this is, of course, to reduce the range of positions that for a single *O* will give a match; for example, 1% of our *O*'s gave the same reading 10 times, 2% 9 times, 19% 6 times, and 32% 5 times of a total of ten trials. This quantitative result is, however, conditioned in part upon the fact that *E* read the scale to the nearest degree, a procedure that, in view of other faults in the apparatus, seemed advisable. The size of the arc that carries the scale is so small that *E* could not read to less than  $\frac{1}{2}^\circ$ , and the distance of the pointer above the scale is so great that unless *E*'s eye is directly above the pointer the error of reading the scale is at least  $1^\circ$ . In the course of our experiments we reduced the error of observation to less than  $\frac{1}{2}^\circ$  by a five-fold extension and enlargement of both scale and pointer; but for the sake of uniformity we continued, as we had begun, to read to the nearest degree. In all other respects the instrument was entirely satisfactory for our purpose; it is easily managed, instantly ready for use, and if its position relative to the source of illumination is permanently fixed is always reliable.

at our disposal would not have cancelled the red. Our experience has shown that a setting of the red reflector at  $25^\circ$  of arc would have given an R that could have been cancelled by all of our *O*'s, and the resultant yellow would have been of lighter tint and better chroma than that which we obtained in this experiment.

<sup>6</sup> In an extended series of observations with a single *O* who adapts slowly in the dark we found that general adaptation upon coming from a light into the dark room did not, after the first three or four minutes, make any sensible variation in the average equations.



As regards the distribution of the matches of normal *O*'s, we give the results of 100 cases. Many more than these were collected; but we have excluded not only those of the preliminary period before we had the apparatus and method of observation under control, but also 10 cases we found to be abnormal in the sense that they were either color-blind or color-weak. When once the final method, which we have described above, was determined upon, we began the collection of the 100 cases, accepting every *O* as he came, and continuing until the hundredth match was obtained. The *O*'s consisted of 36 men and 64 women taken as we found them from the graduate and undergraduate students and from the staff of the department of psychology in Cornell University. We believe that they as a group constitute a fair sample of a university population, and that every individual in the group has normal color-vision. The majority, however, were not tested specifically for color-blindness or color-weakness because indications of abnormality appear in the ordinary course of matching *R* and *G* to *Y*; abnormal *O*'s almost invariably give larger *mv*'s than normal, and they also give an average match that falls at the limits of (or outside) the normal distribution. We, therefore, tested for color-blindness or color-weakness only those individuals whose matches differed markedly from the average, whose *mv*'s were exceptional, and whose color-names in making the match were incorrect; the remainder we assumed to be normal. We give in the following table the distribution of the matches of these normal *O*'s.

TABLE I

DISTRIBUTION OF THE AVERAGE AMOUNTS OF *G* REQUIRED BY 100 OBSERVERS TO EQUATE *R* TO *Y*

Degrees of Arc.....	45	46	47	48	49	50	51	52
No. of cases.....	1	7	18	23	27	17	6	1

Average = 48.48      Median = 49

The difference between groups represented in the table by apposed degrees of arc is, we believe, highly significant. In the first place, the units of measurement were, as we have seen, too large to admit of minute gradations. Furthermore, the *mv*'s from the averages of every ten matches were small; the average of all these was 0.7°, and their median value was 0.6°; 27% of the *O*'s had an *mv* of 0.5° or less, and only 15% of 1° or more. Finally, if only the conditions were kept constant, we found no greater variation in average matches of a single *O* from day to day. We believe also that, in so far as our experiments go, we have determined the limits of normality; we found no case, normal or abnormal, beyond the limit of 45° (i.e., in the direction of less green); and although nine cases fell beyond 52°, they were all abnormal; only one abnormal, a protanope whose average match was 52°, came within the limits of the normal distribution. If, then, the difference between groups is a real difference, and if we have been successful in finding the limits of normality, the range of distribution becomes significant. The mode is at 49° and the limits are 45° and 52°; there is, therefore a greater



range of distribution on the side of less green than on that of more green. In this respect the form of the curve is like one of 59 cases published by Donders, and consequently it may be that this form is typical.<sup>7</sup> But our own curve permits of another interpretation. It will be observed that, with the exception of the extreme groups, the difference in size of groups becomes progressively less as the mode is approached. Furthermore, the average of all matches is  $48.48^\circ$ , and this average falls between the two largest groups which combined equal half the cases. We might regard the curve, therefore, as broad and flat at the mode, and as falling away equally on the two sides. If, as von Kries suggests,<sup>8</sup> this variation among normal *O*'s has a physical cause, i.e., is due to differences in macular pigmentation, there would seem to be no reason why the curve should not approximate to a normal distribution.

We do not wish, however, to overemphasize the form of distribution. The essential feature of our results is that the extent of variation among normal *O*'s has been brought into bold relief. The fact has, of course, long been known to everyone who has observed students, in demonstration of the first law of color mixture, match *R* and *G* to grey; but so far as our reading has gone von Kries is alone in realizing that the variation is distinctive enough to mark off the normal from the anomalous groups.

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<sup>7</sup> F. C. Donders, *Farbengleichungen*, *Arch. f. Physiol.*, 1884, 521.

<sup>8</sup> J. von Kries, in Helmholtz' *Handbuch der physiologische Optik*, ii, 1911, 345.

LII. THE AFTER-EFFECT OF SEEN MOVEMENT WHEN THE WHOLE  
VISUAL FIELD IS FILLED BY A MOVING STIMULUS

By WELLINGTON A. THALMAN

In his study of the after-effect of seen movement A. Wohlgemuth reports a series of 34 experiments whose results he uses to disprove current theories and to support his own theoretical construction.<sup>1</sup> One of these experiments, the 22d, is devised "to ascertain how the after-effect is affected if the whole visual field is filled by an objective movement." The outcome is negative: there is "a total absence of any after-effect." Wohlgemuth, apparently dissatisfied with this finding, repeats the experiment, with identical result. He accordingly concludes that "no after-effect is produced by an objective movement occupying the whole visual field."<sup>2</sup> Later, in his theoretical sections, he reviews and rejects all current theories "either for *a priori* reasons or on account of incompatibility with experimental evidence," and proceeds, on the basis of McDougall's theory of drainage, to the construction of a theory which shall be "in harmony with the ascertained facts."<sup>3</sup> But in relating this theory to the facts *he fails to consider the results of his own Experiment 22.*<sup>4</sup> Yet these results, if they stand, appear to contradict the theory, according to which the phenomenon is in no way conditioned upon size or spread of the stimulus but is wholly dependent on priority of entry and the consequent decrease of synaptic resistance. If the after-effect fails to appear when the whole field is stimulated, the theory must either be discarded or radically revised.

We hold no brief for the theory; but we were not either convinced by the outcome of the critical experiment, which we therefore decided to repeat. We hoped also, by extending the method, by requiring introspective reports of process as well as statements of meaning, and by improving the apparatus and technique, to contribute something toward a complete phenomenology of the illusion.

*Observers.*—We had two groups of O's. The members of the first group were Mr. W. A. Andrews (A), and Mr. S. Takaki (T), graduate students in psychology; Mrs. I. G. Whitchurch (W), graduate scholar in psychology; Dr. F. L. Dimmick (Di), assistant in psychology; and Dr. K. M. Dallenbach (Da), instructor in psychology. We gave these O's a preliminary practice-series that should bring them

<sup>1</sup> *Brit. Journ. Psych.*, Mon. Suppl. 1, 1911, 1-117.

<sup>2</sup> *Op. cit.*, 72, 113.

<sup>3</sup> *Op. cit.*, 98 f. For McDougall's drainage-theory see *Brain*, xxvi, 1903, 153 ff.

<sup>4</sup> The experiment is referred to by number on p. 106; but the "Experiments 20-22" there mentioned should undoubtedly be "20, 21," since we read immediately after of "these two [not three] cases," and since the separate discussion on p. 109 mentions only Exps. 20 and 21. The slip on p. 106 is unfortunate, as it led the author to leave Exp. 22 out of account.

to the pitch of proficiency that Wohlgemuth's *O*'s had attained.<sup>6</sup> They were required to report on both process and meaning. The members of the second group, whom we shall quote only by number, were with two exceptions students in the summer session of 1920. These *O*'s approached the problem without special training, and were asked to report only upon meaning.

*Preliminary Practice.*—We used in the preliminary work a demonstrational model of James' artificial waterfall,<sup>6</sup> with and without a fixation point, in both the vertical and the horizontal positions. The curtain was rotated by hand at two speeds, fast and slow, and the rotation was continued for 5, 10, 15, 20 or 25 sec. There were thus 40 possible experiments; and as every experiment was repeated 5 times, every *O* gave 200 reports upon the illusion. Observation was binocular, at a distance of 25 cm. An adjustable biting-board served to steady *O*'s head and aided him to maintain his fixation.

The results of this practice-series may be summed up as follows.

(1) All *O*'s obtained the illusion of backward movement. (2) The after-effect became more pronounced as practice advanced. (3) The after-effect became more pronounced as the exposure grew longer and the rate of rotation faster. (4) The presence or absence of a fixation point made no difference in the illusion. (5) The after-effect was as marked with horizontal as with vertical movement of the stimulus.

*Repetition of Wohlgemuth's Observations.*—We next set up Wohlgemuth's apparatus and repeated his observations. The apparatus,<sup>7</sup> which consisted of an endless band of printed calico of alternating black and white stripes 5 mm. wide, 180 cm. broad and 110 cm. high, stretched between two rollers, was placed in a dark room and was so illuminated by daylight mazda lamps that no shadows fell upon its surface. The band was driven by an electric motor which, by means of a multiple speed-reducer, moved it downward at a velocity of 12 cm. per sec.<sup>8</sup> The times of stimulation were 30, 45 and 60 sec.<sup>9</sup>

No one of our *O*'s found the entire field of vision filled by the black and white surface, even though he stood so close that his nose actually touched the cloth. Always there was an unfilled margin. It may be that Wohlgemuth's *O*'s failed to report this fact; it may be that Wohlgemuth regarded it as of no consequence. Since, in his second attack upon the problem, he reduced the dimensions of the stimulus-field to a square of 80 cm. side,<sup>10</sup> it is probable that he aimed

<sup>6</sup> On the importance of practice see Wohlgemuth, *op. cit.*, 27, 110.

<sup>6</sup> W. James. *Mind*, O. S. xii, 1887, 517; *Principles of Psych.*, ii, 1890, 245.

<sup>7</sup> *Op. cit.*, 72.

<sup>8</sup> Wohlgemuth does not state his rate of movement. We chose that of 12 cm. per sec. because his experiments on the "influence of the velocity of the stimulating movement upon the after-effect" (Exps. 10-13, pp. 45-52) showed that the optimal rate lay between 6 and 12 cm. per sec. (see Fig. 8, p. 50). These experiments were performed with central vision. Our own experiment involves peripheral vision, and we therefore thought it best to take the upper limit.

<sup>9</sup> We used these long times, although the preliminary practice-series had proved them unnecessary, in order to conform with Wohlgemuth's procedure; see *op. cit.*, 72.

<sup>10</sup> *Ibid.*

only at a relative filling of the field of vision. Wohlge-muth, again, used a fixation point throughout his observations.<sup>11</sup> It is clear that this point, with the wire that held it in place, further violates the conditions of complete objective movement. Our practice experiments had shown that the fixation point is unnecessary, and we therefore discarded it in part of our own work.

We performed three series of experiments with this apparatus. In the first we sought exactly to duplicate his conditions, and therefore introduced a fixation point. A piece of wire carrying a small white square of 5 mm. side was let down from above and ended at the center of the field, about 1 cm. in front of the cloth. In the second and third series the fixation point was removed. In the second, the O's were directed to fixate the surface of the moving field; in the third, they were directed to look through and beyond the cloth, to fixate at 'infinity.'

The following instructions were read to the O's at the beginning of every observation-hour: "Take a position about 10 cm. in front of the center of the screen. When you have done this, close your eyes and wait until the experimenter says 'Ready,' 'Now.' On the 'Now' open your eyes and (a) keep your eyes steadily upon the fixation-point; (b) fixate the surface of the moving field; (c) look through and beyond the screen and fixate at 'infinity.' When the objective movement has ceased and the resulting phenomena have run their course, give a complete account of them."

While the O's had their eyes closed the motor was started, and the screen was allowed to reach its full velocity before the 'Now' was given. At the end of the experiment the screen was instantly stopped; and when the after-effects, if any, had run their course, the O's turned to the description.

Every experiment was repeated twice with every exposure time. The number of times, expressed in percentages, that an after-effect was reported, is shown in Table I.

TABLE I  
SHOWING THE PER CENT. OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED WHEN WOHLGEMUTH'S APPARATUS WAS USED

O	<i>Observed</i>		
	With Fixation-Point	Without Fixation-Point	
		Near Fixation	Far Fixation
A.....	33%	100%	66%
Da.....	16%	100%	33%
Di.....	0%	100%	100%
T.....	66%	100%	100%
W.....	100%	100%	100%
1.....	50%	100%	100%
2.....	50%	33%	100%
3.....	66%	66%	66%
4.....	16%	0%	0%
5.....	0%	0%	0%
6.....	0%	0%	66%
Average.....	36.3%	63.6%	66.6%

<sup>11</sup> *Ibid.*

The after-effect was variously reported as a "slow upward movement," a "rapid upward movement," a "reverse movement," a "rebound," a "jerk upward," a "recoil," a "spring up." The direction of movement in the after-image was always opposite to that of the objective stimulus. The rate, extent, and duration of the movement varied considerably: it was described as rapid, as medium, and as slow; it was local (around the fixation point), general (extending over the entire field), or intermediate between these extremes; it passed through a small angle and was reported as "upward about an inch," or through a large angle and was reported as "upward a foot," or it was intermediate; and it was of short, intermediate or long duration.

The apparatus as employed by Wohlge-muth, with a fixation-point, afforded on the whole the least favorable method for observation of the phenomenon. But even under these unfavorable conditions the *O's*, on the average, reported its presence in more than one-third of the experiments. The individual differences between *O's* was large. Of the trained *O's*, *W* reported the after-effect in every experiment, whereas *Di* failed to observe it in a single case; of the untrained *O's*, 1, 2, and 3 reported an after-effect in at least half of the experiments, whereas 4, 5 and 6 reported it in few or none.

The after-effect was noted in a larger percentage of the cases when the fixation-point was removed. The figures given in Table I show that an after-effect was, on the average, reported by all the *O's* in about two-thirds of the cases.

There is very little difference in the results with near and with far fixation: an after-effect was reported in 63.6% of the experiments with near, and in 66.6% of those with far fixation. In these experiments the individual differences among the trained *O's* was very small: with near fixation every *O* reported the phenomenon after every experiment; with far fixation *Di*, *T*, and *W* continued to report the after-effect after every experiment, whereas *A* and *Da* failed to observe it in  $\frac{1}{3}$  and  $\frac{2}{3}$  of the experiments respectively. The untrained *O's* show greater individual variation. Some gave descriptions which compare with those of the trained *O's*, while others, like Wohlge-muth's *O's*, failed to observe an after-effect.

The effect of practice and training is clearly marked. Those *O's* who had taken the practice series reported the after-effect more frequently and more consistently than the untrained *O's*. Two of the untrained *O's*, 1 and 2, gave reports and percentages which resemble those of the practised group. These *O's* were both psychologists, and their general training in observation sufficed to differentiate them from the unpractised and untrained group.

In a number of the experiments in which no after-effect was observed the *O's* stated that the objective stimuli fused as in color-mixing; the white and black lines disappeared, and the background became a uniform grey. Fusion was more frequently reported with fixation, when the eyes were strained and attention was concentrated upon the white square, than without.

We conclude from our repetition of Wohlge-muth's experiments (1) that the apparatus is unsuited to the problem. For (a) the entire visual field is not covered by an objectively moving stimulus; (b) in order to approximate this condition the *O's* must stand so near the moving field that the black and white tend to fuse; (c) fusion is especially noticeable in experiments in which a fixation point, with



the resulting strain of convergence and concentration of attention, is introduced; and (d) the presence of this point and of its wire support violate the conditions of complete objective movement; the point of clearest vision and maximal attention is stationary. We find (2) that even under the unfavorable conditions of Wohlgemuth's procedure the after-effect may be observed. The reports vary considerably, but correlate roughly with the training and experience of the O's. Trained O's and experienced O's without special training report a reverse movement in about half of the experiments. We note (3) that if the problem of the after-effect of complete visual movement is to be successfully attacked we must change the apparatus: (a) the whole field of vision must be stimulated; (b) the fixation point must be eliminated; and (c) the distance of O from the moving field must be large enough to rule out eye-strain.

*Principal Experiment.*—The apparatus which we finally constructed consisted of a large cylinder 1.7 m. high and 1.25 m. in diameter.<sup>12</sup> On the inside we glued heavy white architect's paper on which had been drawn in carbon ink at intervals of 7 mm. black lines 7 mm. in width. The cylinder was supported from above by a shaft projecting from a ball-bearing swivel joint.<sup>13</sup> It was rotated by an electric motor at two rates of speed, which we shall hereafter designate as 'fast' and 'slow.' At the fast rate the cylinder moved with a velocity of nearly 60 cm. per sec., at the slow rate with a velocity of approximately 30 cm. per sec.<sup>14</sup> The times of stimulation were 5, 15 and 25 sec.<sup>15</sup> The apparatus was placed in a dark room, so that the illumination was under control. The cylinder was lighted from within by a daylight mazda lamp placed above and slightly in front of the O. There were no shadows, and the illumination was uniform over the whole field of vision.

The O sat upon a stool at the centre of the cylinder. A biting board, rigidly supported from the floor, was employed to insure a constant distance of 29 cm. between O's eyes and the cylinder wall.

<sup>12</sup>An apparatus of this form was used by J. Aitken in 1878 (*Proc. Royal Soc. Edin.*, x, 1878, 40 ff.; *Journ. Anat. Physiol.*, xiii, 1879, 322 ff.) with negative results. The cylinder was rotated about the O and then lifted; no after-effect was observed upon the surrounding objects. The sole uniform experience was a disagreeable, sickening effect. E. Budde employed the same apparatus in 1884 (*Arch. f. Anat. u. Physiol.*, 127 ff.). The experiences set up were so trying that the experiments were soon broken off.

<sup>13</sup>This is a part of the apparatus for localisation of sounds described by M. Bentley, this JOURNAL, xxiii., 1912, 509.

<sup>14</sup>The exact rates were respectively 57.3 and 34.1 cm. per sec. Cf. Wohlgemuth, *op. cit.*, 28, par. 8.

<sup>15</sup>We had found, in our preliminary experiments, that the short exposure times were as effective as the long to produce the after-image of movement. Nausea and dizziness, as the two earlier authors found (footnote 12), result from the stimulation of the cylinder; but they may be eliminated by shortening the time of exposure. With the intervals used, only two O's reported these experiences; and in neither case were they sufficiently strong or unpleasant to force a withdrawal from the experiment. As the experiment progressed these O's became habituated, and (like the other O's) were unaffected even by the 25 sec. stimulation.

This distance, chosen after trial, proved very satisfactory; there was no noticeable eye-strain.

The cylinder was stopped by a brake operated from *E*'s desk by a rigid lever-arm 3 m. in length. The effect was absolute and instantaneous. Great care was taken with this part of the apparatus; and records by the graphic method showed that the arrest of motion was sharp and abrupt; there was no indication either of continued movement or of rebound when the brake was applied.

There was no fixation point. The *O*'s were directed in half of the experiments to fixate the surface of the cylinder and in half to look through and beyond the screen, i., to fixate at 'infinity.'

We had, then, two velocities, a fast and a slow; three durations, 5, 15, and 25 sec.; and two fixations. We had also separate instructions for meaning and for process. The series were repeated 5 times.

The *meaning* instructions were as follows: "When the signal 'Ready' is given close your eyes. The cylinder will then be set in motion. When the proper velocity has been reached the signal 'Now' will be given. On the 'Now' open your eyes and (a) fixate the surface of the screen at a point directly in front of you; (b) look through and beyond the screen, fixate at 'infinity.'"

In the preliminary experiments you reported, among other things, an after-effect of various degrees of intensity, duration, and extent. You are now to characterize this perception as fully as you can. Designate the degree of movement in the after-image on the following scale:

0. no after-effect
1. after-effect poor
2. after-effect fair
3. after-effect good
4. after-effect very good
5. after-effect excellent

When the objective movement has ceased and the resulting phenomena have run their course, give an account of them."

In the instructions for process the same general *Aufgabe* regarding signals and fixation was laid down. The specific instructions for meaning were omitted, and the following sentences were added: "In the preliminary experiments you reported after-images of movement. In this experiment you are to concentrate upon description. When the objective movement has ceased and the resulting phenomena have run their course, describe the mental processes in strictly psychological terms."

Only the practised *O*'s observed in this part of the experiment. The order in which the series were presented was different for every *O*, as follows:

Series	Observer				
	A	Da	Di	T	W
Meaning, near fixation.....	3	h a p	2	1	4
Meaning, far fixation.....	4	h a z	3	2	1
Process, near fixation.....	1	a r	4	3	2
Process, far fixation.....	2	d	1	4	3

*Results.*—The results of the experiments upon 'meaning' appear in Tables II and III.

TABLE II

SHOWING THE NUMBER OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED, DISTRIBUTED ACCORDING TO THE POSITION OF FIXATION, THE RATE OF THE OBJECTIVE MOVEMENT, AND THE DURATION OF THE STIMULATION

O	Fix- ation	Rate	Duration				Total Near or Far	Total Slow	Total Fast
			5	15	25	Total			
A	Near	S	2	3	4	9	13	20	7
		F	1	1	2	4			
	Far	S	3	4	4	11	14		
		F	1	1	1	3			
	Total		7	9	11	27			
Da	Near	S	5	5	5	15	28	28	27
		F	4	4	5	13			
	Far	S	3	5	5	13	27		
		F	4	5	5	14			
	Total		16	19	20	55			
Di	Near	S	1	1	4	6	12	9	7
		F	1	2	3	6			
	Far	S	0	1	2	3	4		
		F	0	0	1	1			
	Total		2	4	10	16			
T	Near	S	4	5	5	14	27	25	26
		F	3	5	5	13			
	Far	S	2	4	5	11	24		
		F	4	4	5	13			
	Total		13	18	20	51			

O	Fix- ation	Rate	Duration				Total Near or Far	Total Slow	Total Fast
			5	15	25	Total			
W	Near	S	5	5	5	15	30	30	30
		F	5	5	5	15			
	Far	S	5	5	5	15	30		
		F	5	5	5	15			
	Total			20	20	20	60		
Summary			58	70	81	209	110/99	112	97

Table II summarises the cases in which the after-effect was reported by the *O*'s according to the position of fixation, the velocity of the objective movement, and the duration of the stimulation.

Under our conditions a negative after-image of movement was perceived by all *O*'s. This result was corroborated by those of the unpractised group, which now comprised 12 *O*'s. A series of five experiments, with near fixation, fast velocity, and a 25 sec. exposure, was taken with every member of this group. The meaning instructions alone were used. Four *O*'s reported the perception of reverse movement in every experiment; three in 80%; one in 60%; three in 40%; and one in 20% of the experiments. It should be recalled that all investigators have found that the after-effect becomes more pronounced with practice, and that this group approached the problem without the slightest training.

The results obtained from the two groups of *O*'s in this part of the experiment indicate that an after-effect occurs when the whole visual field is filled by an objective moving stimulus. Compulsory conditions, however, were not obtained for every *O*.

In the practised group compulsory conditions were obtained for three *O*'s: *W* reported the after-effect in every experiment, no matter how (within our limits) the variables were arranged; *T* reported it in every case when the stimulation had been extended to 25 sec.; and *Da* in every case when the fixation was 'near' and the velocity 'slow'; also in every case, no matter where the fixation or what the velocity (within our limits), when the stimulus was extended to 25 sec.

Compulsory conditions were not obtained for *A* and *Di*. Their reports, however, indicate the approach of compulsion: an increase in time of stimulation is paralleled by an increase in reports of the after-effect. For the 5 sec. exposure these *O*'s report the after-effect in 7 and 2 cases respectively; for the 15 sec. exposure, in 9 and 4 cases; and for the 25 sec. exposure, in 11 and 10 cases. Had our variable conditions been slightly extended, or had the time of stimu-

lation alone been lengthened,<sup>16</sup> it is very probable that we should have obtained compulsory conditions for them also. Owing to the short duration of the summer session it was impossible to carry the experiment further.

The summary at the foot of Table II shows that long stimulation, near fixation, and slow movement are, within our limits, the most favorable conditions for the perception of the illusion. As in the data of A and Di, so also in the results of the other *O*'s, the after-effect is most frequently reported when the exposure is extended to 25 sec. The summary also shows that the after-effect is reported 110 times with near fixation, 99 times with far; 112 times with slow rotation of the cylinder, 97 times with fast. The differences here are slight, and probably of little significance, since they are primarily due to the results of a single *O*: to those of Di in the matter of position of fixation, and to those of A in that of rate of rotation. Our conclusion is that position of fixation and rate of objective movement are irrelevant moments, and that the conditioning factor is the duration of stimulation.

These conclusions are confirmed by the *O*'s estimate of the cognitive clearness of the after-effect, which is presented in Table III.

These results parallel those of Table II. The *O*'s who report comparatively few cases of an after-effect place them for the most part at the lower end of the scale, whereas those who report many cases distribute them toward the upper end.

The results regarding the variable moments were likewise confirmed: the distribution upon the clearness scale for 'near' and 'far' fixation, and for 'slow' and 'fast' objective movement, shows no relevant differences, whereas the distribution according to the duration of stimulation shows a shift from the lower end for the short exposures to the high end for the long exposures. Even in the case of W, who reported the phenomenon in every experiment, the clearness of the after-movement shifts with the longest exposures to the highest degrees.

An analysis of the introspective reports yields the following results:

(1) The movement, given in the exposure and in the after-effect, is at times subjectified. Some *O*'s report that they revolved as on a swivel chair, and that the screen was stationary. The after-effect in these cases is merely a reversal, as in the rotary chair experiment, of the direction of the subjective movement. Examples are: (A) "Swimming sensations; I revolved in direction opposite to that of the cylinder. When the objective movement stopped, I rotated in the opposite direction;" (Di) "Pressures in head; feel of moving backward; this feeling kinaesthetic not visual;" (W) "Seemed as if I were doing the moving; felt as if I were moving to the right" (rotation of cylinder was to left).

<sup>16</sup> Wohlgenuth found that short stimulations of 5 sec. or less (Exp. 31, p. 85) failed to produce an after-effect of movement. He used exposure times varying from 20 sec. to 3 min. The majority of his experiments were performed with times lying between 30 and 60 sec. Our preliminary experiments showed that it was possible to obtain the illusion with the shorter times. The obvious advantage of using these shorter exposures lies in the fact, mentioned above, that the disagreeable and distracting effects, nausea and dizziness, caused by the rotating cylinder, were reduced to a minimum.



TABLE III

SHOWING THE NUMBER OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED AT THE VARIOUS LEVELS OF COGNITIVE CLARNESS, AND A DISTRIBUTION OF THE CASES ACCORDING TO THE POSITION OF FIXATION, THE RATE OF MOVEMENT AND THE DURATION OF THE STIMULATION

O	Degree	Total No. Cases	Fixation		Rate		Duration		
			Near	Far	S	F	5	10	15
A	0	33	17	16	10	23	13	11	9
	1	10	5	5	8	2	3	4	3
	2	8	3	5	7	1	2	3	3
	3	5	3	2	4	1	2	1	2
	4	3	2	1	1	2	0	1	2
	5	1	0	1	0	1	0	0	1
Da	0	5	2	3	2	3	4	1	0
	1	23	15	9	13	11	11	7	6
	2	11	4	7	3	8	3	3	5
	3	12	5	7	8	4	1	8	3
	4	6	3	3	3	3	1	1	4
	5	2	1	1	1	1	0	0	2
Di	0	44	18	26	21	23	18	16	10
	1	13	9	4	9	4	2	2	9
	2	0	0	0	0	0	0	0	0
	3	2	2	0	0	2	0	1	1
	4	1	1	0	0	1	0	1	0
	5	0	0	0	0	0	0	0	0
T	0	9	3	6	5	4	7	2	0
	1	29	13	16	19	10	9	11	9
	2	19	13	6	6	13	3	7	9
	3	3	1	2	0	3	1	0	2
	4	0	0	0	0	0	0	0	0
	5	0	0	0	0	0	0	0	0
W	0	0	0	0	0	0	0	0	0
	1	3	2	1	2	1	3	0	0
	2	11	7	4	8	3	7	3	1
	3	3	1	2	3	0	1	1	1
	4	18	9	9	9	9	8	8	2
	5	25	11	14	8	17	1	8	16

(2) The black lines are at times seen in perspective; they are objectified, and appear as rods set at various distances from the eyes. All the O's report experiences of this kind. Examples are: (A) "The lines stood out in perspective. Three in a group; that is, three were equally near, then the next three were back, next three close, and so on;" (Da) "At times the lines were seen in perspective, like black rods arranged at various distances in front of the observer;" (Di) "Lines stood in perspective;" (T) "The projection-effect was very

conspicuous; every second line seemed to stand out;" (W) "Once during the experiment there was a projection of the lines." "Twice during this experiment I noticed a projection; lines stood out in model-effect on grey background."

(3) The lines at times lose their shape and outline. Patterns of varying degrees of complexity appear upon the ruled surface. Simple changes (the lines become fringed and nodulated) and complex changes (large diamond and zig-zag effects) are of about equally frequent occurrence. The appearance of the simple and complex patterns can be correlated roughly with the slow and fast velocities. Examples are: (A) "The lines during motion became small squares, with vertical and horizontal cross-lines. These patterns appeared first at the center of the field of vision, and radiated until the entire field was covered;" (Da) "During the objective movement the field became spotted; the lines lost their regular outline;" "lines became nodulated;" (T) "Grey fringe to the right edge of the lines;" (W) "Lines became ripply;" "lines interwoven into complex pattern."

(4) Color effects are produced by the colorless stimuli. The hues are brilliant and filmy. They flicker and fluctuate. Sometimes they change as in the 'flight of colors;' sometimes antagonistic colors alternate; sometimes two colors (always antagonistic) appear together. Thus: (A) "The lines during motion became brilliantly colored;" (Da) "Left edge of lines blue, right edge yellow;" "lines alternately tinged with yellow and blue;" (T) "During objective movement the lines became wavy. The left side was colored, quality changing from yellow to red, to blue, to green, etc. The wavy figures instable, rapidly changing form and color;" (W) "During motion the lines became tinged with blue."

(5) The lines fuse as in color-mixing into a uniform background, in which case there is no after-effect; or into a flickering background as when the rotation of the color-mixer is too slow, in which case the after-effect is either not observed or if observed is reported as of low degree. Thus: (A) "Lines blended; there was no after-effect;" (Da) "Lines were nodulated; seemed to be stationary blinking spots" (no after-effect of movement was reported); (Di) "Faded to a shimmering gray." Di frequently reported this phenomenon; (T) "Lines became thicker in width at some points and thinner at others. Positions of thinness and thickness not constant, continually changing;" (W) "Toward end the whole thing became a mottled grey" (the after-image of movement was reported as poor).

(6) As Aitken and Budde discovered, stimulation from within the cylinder produces a widespread organic and muscular response. Dizziness and nausea are frequently reported, and tend to obscure the process-configuration. Examples are: (A) "Swimming in head;" (Da) "Strain sensation in muscles of eyes and pressure within head. Dizziness and an organic stir-up;" (Di) "Intense pressure in head, eye strain, and slight dizziness;" (T) "Very strong strain in eye muscles and dull pressure in head;" (W) "Very dizzy and nauseated, pressure sensations in head."

The occurrence of the after-effect can be directly correlated with the arousal of these phenomena. Di, who reports the after-effect least often, reports dizziness but once; W, who reports the after-effect in every experiment, reports dizziness most frequently. This correla-

tion does not indicate, however, that 'dizziness' is essential to the appearance of the after-effect. No *O* reports dizziness every time that he reports the after-movement. It is only in the early experiments that the correlation, even with *W*, invariably occurs. It appears, therefore, that the optimal conditions for the after-effect are also optimal for the perception of dizziness. It is not until these perceptions became habituated that the process-configuration becomes clear.

(7) The after-effect is reported in precisely the same terms as in our previous experiments. The same perception, an after-image of movement opposite in direction to that of the stimulus, and differing only in extent, is obtained when the entire visual field is filled by an objective moving stimulus as when, with James' waterfall apparatus, the stimulus is limited to a small area of the retina.

(8) The introspections at first include kinaesthetic and organic complexes. As the experiments progress and the *O*'s become accustomed to the experiences, the inessential elements drop away, leaving a residue of pressure and visual sensations.

The pressure sensations within the head never entirely disappear. They are always present when the after-effect is reported.

The visual configuration reported by three of the *O*'s—*A* and *Di*, even in the experiments under process-instruction, do not get beyond statements of meaning—is a qualitative and temporal, and perhaps spatial, integration. Examples are: (*Da*) "Immediately after the objective movement ceased the field became momentarily a blur and grayish; then the vertical lines stood out distinctly again;" "The gray blur was of short duration. On the side of meaning movement rapid;" "When objective movement ceased, field seemed to be covered by a gray cloud; this lasted for a short time; then the vertical lines appeared, distinct in outline. On the side of meaning movement slow;" "When objective movement ceased there was a momentary gray blur, which was followed by distinct vision. On side of meaning a rebound;" (*T*) "When the objective movement stopped there was a grey; the lines became wider; and then they became distinct again;" "When the objective movement stopped I saw gray like a film;" "When the objective movement stopped the lines momentarily appeared clear in outline, then expanded to right. Over these lines were gray lines; these were indefinite and instable" (reported movement as of the 2nd degree); (*W*) "When movement ceased lines blurred into a gray;" "After objective movement ceased each line seemed to become broader; meaning, movement to right;" "When objective movement ceased a gray screen appeared; this seemed to move to right."

#### SUMMARY

(1) We divided our *O*'s into two groups; the one group was given a practice series before the main experiments were undertaken, the other group approached the problem naïvely.

(2) Repetition of Wohlgemuth's work showed:

(a) that his apparatus is unsuited to the problem;

(b) that even under unfavorable conditions the after-effect is reported in a large percentage of cases by *O*'s of both groups. Individual differences are large, and the effect of practice and training is clearly evident;

(c) that the apparatus, if the problem of the after-effect of complete visual movement is to be successfully attacked, must be variously modified.

(3) Experiments with a large cylinder, constructed to meet the requirements of complete visual stimulation, showed:

(a) that the after-effect is observed when the whole visual field is filled by an objective moving stimulus;

(b) that position of fixation and rate of objective movement (within our limits) are irrelevant moments, and that the conditioning factor is the duration of stimulation;

(c) that the characteristic processes of the after-effect are pressure sensations within the head and a qualitative, temporal, and perhaps spatial integration of visual sensations.

(4) Wohlgemuth's failure to attempt a reconciliation of his theoretical views with the negative results of his Exp. 22 turns out to be of no consequence: not because the results of that experiment are unimportant, but because under strict conditions they are positive. As it happens, Wohlgemuth's two negatives amount to an affirmative.

## BOOK REVIEWS

*The religious consciousness: a psychological study.* By J. B. PRATT.  
New York, The Macmillan Co. 1920. pp. viii, 486.

This very substantial and comprehensive addition to the growing literature of the subject furnishes new evidence of the pre-eminence of American scholars in the psychology of religion. The reader may find here in very clear and attractive form the results of this twenty-one-year old science. The chapters arrange themselves into four groups. The first four may be considered as introductory, dealing with the definition of religion, the nature of the psychology of religion, the significance of the subconscious and the relation of society and the individual. The next five chapters cover the topics and materials presented in several books beginning with Starbuck's. The discussions treat of the religion of childhood, adolescence, conversion and revivals. Six chapters follow which contain perhaps the most original and vital pages of the entire book. The subjects are belief in God and immortality, the nature of cults, and the phenomena of worship and prayer. The last five chapters give an exposition of mysticism and a criticism and estimate of its central principles and practices. Certain problems in each of these groups of chapters will be considered in this review.

The preface emphasizes the purpose of the author to give a description of the religious consciousness in a purely objective way without the bias of any point of view. This he succeeds in doing unless it is in certain passages touching upon the work of some writers whose point of view is avowed and different. The definition of religion given is suggestive of the procedure. "Religion is the serious and social attitude of individuals or communities toward the power or powers which they conceive as having ultimate control over their interests and destinies." The attractiveness of this definition lies in the use of the term "attitude" instead of belief, and in the recognition, though somewhat faint-heartedly, of the social character of religion. The difficulty which the author has with this term 'social' gives the impression that it is forced upon his attention by the drift of current thinking, but without his full consent or satisfaction. He still regards the social as in some way opposed to the individual, but the relation is not clearly set forth. For example, he would seek the origin of religion "within the subjective needs of human nature," but "the religious consciousness inevitably considers its religion objective as well as subjective." Subjective here apparently means subjective to the individual, and objective means more objective than writers like Durkheim would imply by the word social. By thinking of the social as subjective the author fails to recognize the force which the expression "social consciousness" has for those writers whose use of it he criticizes. In connection with the definition it is indicated that the "power or powers" referred to may be regarded as equivalent to the "Determiner of Destiny." The latter expression is the more frequently used. The value of this term evidently lies partly in its vagueness. It is not intended to designate exclusively a personal object of the religious attitude, although it might comprehend that with the



non-personal types. "To describe the workings of the human mind so far as these are influenced by its attitude toward the Determiner of Destiny, is the task of the psychology of religion."

The second group of chapters gives an excellent restatement of the development of religion through childhood and adolescence. The author holds with practically all present-day psychologists that there is no one religious instinct. He rejects the conception of childhood which holds that "Heaven lies about us in our infancy!" He says "*Earth* lies about us in our infancy," and the baby is a little animal. Family influence is the most potent in religious development, but all social contacts make their contribution. Adolescence is "the flowering time for religion." It is, however, full of ups and downs, of storm and stress. The individual gains in power and selfhood but at the same time experiences bewilderment and depression. The essential thing about conversion is the unification of character, the achievement of a new self. Many conversion-experiences are quoted from various religions and sects. Revivalism is criticized adversely for the most part, with an analysis of its methods of emotional, mob suggestion.

The third group of chapters introduces more new observations and discussions than any other part of the book. The two main questions raised concerning God are: why do people continue to believe in God? and what are the psychological factors that influence or determine the meaning of that term? It is observed that the idea of God is generally not very clear. A good deacon is quoted as confessing that his idea of God was "a kind of oblong blur." Sociological influences, such as the form of family and political control, have much to do with impressing the fatherly and kingly patterns upon the idea. The psychological influences are largely in the field of sense and imagination. Images, pictures, lights and sounds are conspicuous. Both sensuous and conceptual factors belong to the idea of God, though the vividness and definiteness of these factors vary greatly in individuals and in the different moods of the same person. There is found here a certain justification for the anthronomorphic image of God which so many persons have struggled to eliminate. The image may be merely symbolic, but it has great power over the emotions. The imaginative and the conceptual aspects of the idea of God, as of other ideas, are not necessarily in conflict but are normally, though with varying emphasis, present and vital. The reasons why people believe in God are put under four types, authoritative, reasoned, emotional and volitional. These types are illustrated by instances received through questionnaires. Under the reasoned type appear the influences of the traditional arguments, the cosmological and the teleological. Others reason from the practical effects for good upon the lives of those who believe. The emotional type seems to be more numerous than any of the others. This includes cases of the "experience of God's presence," as among the mystics. The volitional type embraces those whose faith arises from a more or less deliberate "will to believe." The same four types of belief are cited with reference to immortality. Belief in immortality is regarded as coming earlier and as being less dependent upon instruction than belief in God. It is more a matter of feeling and will, of instinctive impulse toward self-preservation. There are, however, many people who do not care for immortality, and there seems to be a lessening desire for it through the last five hundred years because, partly at least, this world has become more attractive.

A good account is given of the cult and its function. The author's travels in India and his observation of the ceremonies of the Roman Catholic church have enabled him to appreciate the poverty and barrenness of Protestant services so far as symbolism is concerned. Accepting the genetic account of the writers who find the origin of the cult in social activities, Professor Pratt seems to fail to recognize the way in which the sense of the group may also supply the equivalent of the "Determiner of Destiny," and how such writers as Durkheim, Jane Harrison, and Cornford have shown the dependence of the cosmic upon the social consciousness. The latter point is dismissed too easily (p. 261). The use of images and idols in religious ceremonies comes out in a new light when viewed in reference to the value of these as means of enlivening religious faith and feeling. "The sensuous representation reinforces the reality-feeling. Much more may thus be said in defence of the practice of 'idolatry' than most of us have been brought up to suppose." Further, the activities of the cult have a similar value for the participant. "The feeling of these acts is a considerable part of the religious emotion." To be most potent in one's experience it must have been cultivated in childhood. An interesting comparison is made between the objective worship of the Roman Catholic church and the subjective worship of Protestantism. The Catholic church occupies itself with the worship of God. The Protestant seeks the salvation of souls. It is immaterial to the former whether a congregation is present, but Protestantism is dependent upon the presence of people. The candles of the Catholic churches are for God; the flowers of the Protestants are for the people to see. The author elaborates this contrast very convincingly. It is doubtful, however, whether he has not overstressed the subjective phase of Protestant services. Does any one "attempt to produce merely subjective religious effects?" Here an effect achieved with the group seems to be considered as much subjective as the emotional state of an individual. But it often happens that the common will of an assembly deliberating upon measures of public welfare communicates itself to the individuals as an objective, super-individual judgment. It does not seem quite exact to designate that consensus of opinion and feeling as "subjective." The author admits that "objective worship of the sort that aims to please the Deity is a thing of the past" (p. 308). His description of objective worship other than this is not quite clear. He surely does not mean that men can worship "cosmic forces." It is likely that the author has not yet spoken to his own satisfaction on this point. Having so qualified his "Determiner of Destiny" as to render it unable to take the rôle of God as commonly conceived, he does not adequately provide for the central factor in religious worship. The same problem concerning the meaning of the word subjective affects the discussion of prayer. "If it be true both that the subjective value of prayer is very great and also that it is the only value which prayer possesses, this latter fact should be assiduously kept secret."

Professor Pratt gives more space to mysticism than is found in any other general treatise on the psychology of religion. Mysticism is defined "as the sense of the presence of a being or reality through other means than the ordinary perceptive processes or the reason." Two types are distinguished, the mild and the extreme. Extensive quotations are cited to set forth their characteristics. The sense of presence is the largest factor. This is often most vivid when the mystic

is alone on the hills or in the woods. It is susceptible of cultivation; and the literature of mysticism is much occupied with the technique and discipline by which its goal is reached. Pratt recognizes the fact that many mystics are abnormal and that their quest for this great experience has brought the illusion of attaining it. Still he does not share the views of Murisier and Leuba in this respect, but holds that mysticism has positive value and adds assurance and vividness beyond ordinary religious experience. In the discussion of mysticism, as in other parts of the book, the author shows his close affinity to William James.

The style is clear and entertaining. The use of first-hand material secured by questionnaires adds concreteness and vividness, but there can be little scientific value in trying to employ the method of percentages on 170 replies concerning prayer or belief in God! The abundant references and extensive footnotes open into attractive elaborations of the discussions in the text. While scarcely prepared in the manner of a text-book, the book will undoubtedly be used for that purpose and as collateral reading. For the general reader and for the teacher and preacher of religion it affords one of the most adequate treatments of the subject to be had. It is critical and discriminating, but it is also decidedly friendly and constructive.

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*Das jüdische Volk und seine Jugend.* By S. BERNFELD. Wien, R. Löwit Verlag, 1920. 149 pp.

The first half of this book is devoted to a negative criticism of the aims and results of European education. Since the child is looked upon as an adult 'in the rough,' whom it is the function of the educational process to polish into adulthood, the results of European education are bound to be an unnatural forcing of the child's development and the destruction of the cultural possibilities inherent in youth.

The second half of the book gives a vivid presentation of a system of education to prevail half a century hence in Palestine, which is conceived as possessing by that time the seat of a Jewish commonwealth. The national constitution provides for the complete support and control by the state of the entire youth of the country from birth to the age of twenty. The young people are concentrated in settlements in different parts of the country, under the guidance of a psychologically and medically trained corps of educators, whose office is merely to observe the development of their charges and to be ready to aid them in their spontaneous efforts to acquire any particular discipline or skill. Within these communities the great thinkers and artists of the country, pursuing their work in a congenial atmosphere and drawing interested groups around them, create centers for cultural growth.

This Utopia can only be realized in a country whose productivity, necessarily high, accrues not to the individual but to the state, and among a people who appreciate youth for its own sake,—therefore in a Jewish Palestine.

S. FELDMAN

## NOTES

### A NOTE ON "VOCALITY"

In a recently published paper,<sup>1</sup> Dr. Weiss attempts to redefine the 'attribute' of vocality. He seeks to identify judgments of vowel-character with that attribute which is describable in terms of mellowness and shrillness, and asks all investigators to use the name "vocality" for this characteristic.<sup>2</sup> It would seem advisable to examine critically the foundations upon which such a classification may be based.

A psychophysical study of the tonal attributes brings forth evidence, cumulative rather than direct, that judgments of vowel-quality are not made upon a purely attributive basis, but result from the observer's experience in the use of spoken language.<sup>3</sup> This Dr. Weiss tacitly admits when he says that "when a subject is asked to discriminate the vowel character of a fork tone, the reaction is one that is already well established in the ordinary (speech) habits of the individual."<sup>4</sup> But he regards these habits as merely selecting the conventionalized points in an already existent continuum, which may be described either in vocal terms or in terms of mellowness and shrillness. Moreover, these conventionalized points occur in an octave relationship, because the process of conventionalization picks, as the vowels of any language, combinations of mellowness and shrillness that are separated by equal sensorial steps, corresponding with logarithmic intervals in the series of physical frequencies. The octave-relationship is, then, to be regarded as manifestation of the Weber-Fechner Law.<sup>5</sup>

My own study of the tonal attributes shows that the limen for vocal judgments does not follow Weber's Law.<sup>6</sup> Of the tonal attributes, volume is the only one that gives indication of following this law.<sup>7</sup> A comparison of the differential limens for volume and for vocality shows that the former are uniformly the larger of the two, even though the vocal judgment is more difficult and less immediate than is the judgment of volume. This relationship, of course, precludes the possibility that the vowels are judged upon a basis of volume, that is to say, upon the basis of the characteristic of pure tones that obeys the logarithmic law.

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<sup>1</sup> A. P. Weiss, The Vowel Character of Fork Tones, *Amer. Jour. of Psychol.*, 31, 1920, 166 ff.

<sup>2</sup> *Op. cit.*, 191.

<sup>3</sup> G. J. Rich, A Study of Tonal Attributes, *Amer. Jour. of Psychol.*, 30, 1919, 131 ff. Although Dr. Weiss' paper appeared later than mine, it would seem that he wrote it at an earlier date.

<sup>4</sup> *Op. cit.*, 192 f.

<sup>5</sup> *Op. cit.*, 190 f.

<sup>6</sup> G. J. Rich, *op. cit.*, 139.

<sup>7</sup> *Op. cit.*, 149 ff. Also A Preliminary Study of Tonal Volume, *Jour. of Exper. Psychol.*, 1, 1916, 13 ff.

When he invokes the Weber-Fechner Law as an explanatory principle,<sup>8</sup> Dr. Weiss narrows his conception of vocality to that of a characteristic which shows the octave-relationship found only by Köhler. The trend of recent experimental work tends to show that the vowels of a language (Dr. Weiss' 'conventionalized vocality combinations') seldom occur an octave apart.<sup>9</sup> Köhler's experimental situation was such that, as Stumpf has suggested,<sup>10</sup> his observers may well have tended to judge as the 'pure vowels' the same tonality occurring in successive octaves. This would seem to be a sufficient explanation of the relationship found by Köhler, especially in view of the subsequent lack of verification.

On the other hand, vowel-character is considered by Dr. Weiss as a continuum which is to be described in terms of mellowness and shrillness, terms which are descriptive of pitch-brightness. In so far as the limens for pitch-brightness are in all cases lower than the vocal limens, this attribute may serve as the basis for the perceptual judgments of vowel-likeness.<sup>11</sup> The successive character of the qualities that form the pitch-brightness series is definitely in line with what is seemingly the only constant factor in the results of the many investigations of vowels, the order in which the vowels follow one another as one ascends the musical scale.<sup>12</sup> But the possibility that vocal judgments are based upon pitch-brightness does not necessarily identify whole vowel-series within a range of little over an octave. *Op. cit.*, 133. them with the latter attribute. The very fact that the limens for vowel-character and for pitch-brightness are vastly different, both in magnitude and in course,<sup>13</sup> is evidence that the vocal judgment is not made singly and unequivocally upon a basis of pitch-brightness, that is to say, it is not merely a judgment of pitch-brightness in a 'conventionalized combination.' There is not at present sufficient evidence to warrant the assertion that the vocal perception (or 'habit') is a single attributive continuum describable in non-vocal terms, in terms of one or more of the attributes of pure tones.

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<sup>8</sup> The Weber-Fechner Law would explain fully as satisfactorily the occurrence of the pure vowels (the 'conventionalized vocality combinations') at any intervals other than the octave, *e.g.*, at intervals of fifths, thirds, etc.

<sup>9</sup> D. C. Miller, *The Science of Musical Sounds*, 1916. H. Schole, Über die Zusammensetzung der Vokale U, O, A, *Arch. f. d. ges. Psychol.*, 13, 1918, 12 ff. C. Stumpf, Die Struktur der Vokale, *Sitzungsber. d. Preuss. Akad. d. Wissensch.*, 1918, I Halbband, 333 ff. G. J. Rich, A study of Tonal Attributes, *Amer. Jour. of Psychol.*, 30, 1919, 131 ff. Dr. Weiss' own experimental results fail to show an octave relationship.

<sup>10</sup> C. Stumpf, Über neuere Untersuchungen zur Tonlehre, *Ber. ü. d. 6 Kong. f. exper. Psychol.*, 1914, 305 ff.

<sup>11</sup> G. J. Rich, *op. cit.*, 138 f; 144 f; 154 ff; 164.

<sup>12</sup> Thus, unpractised observers may report, in proper succession, the

<sup>13</sup> See Footnote 11.



### CONSCIOUSNESS IN THE SIAMESE TWINS

Bolton's account of the Siamese twins in 1830 (G. B. Bolton, *Philos. Trans.*, 1830, 177-186) suggests an overlapping of consciousness which is not unlike the overlapping of two hysterically separated consciousnesses. It will be recalled that the twins, Chang and Eng, were possessed of entirely distinct personalities and sensory equipment; only when the band uniting them was punctured by a needle at its very middle did both shrink, whereas half-an-inch to either side the prick brought response in only the one or the other. Nevertheless Bolton thought he found some community,—an overlapping "unconscious" and a distinct "conscious" perhaps he would have said to-day! He wrote:

"They always fall asleep at the same moment, and it is impossible to wake one without also arousing the other. When they were at Boston, Doctor Skey, Surgeon General to the British Army, entered their bed-room at midnight on three successive nights when both were asleep: on each occasion he touched one and was answered by the other, both awaking at the same instant, inquiring why they were disturbed.

"The experiment has also been repeated in this country, and with the same result. On my tickling one of them, the other told me to desist, though he stated that he did not feel the touch, and it was quite clear that he could not see me tickle his brother.

"On their voyage to England one of them had the tooth-ache, during three days and nights, and suffered great pain, with loss of sleep, which last evil was shared by his brother, both remaining awake. On the 16th of December Mr. Hale went into their bed-room when they were asleep. Eng was restless and tossing about in bed, while Chang was screaming. He awoke them, and on inquiring what ailed them. Eng replied that he was dreaming about his mother, and Chang said that a man was cutting off the long hair from his head. These different dreams appear to have occurred simultaneously" (p. 184).

E. G. B.

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## THE STIMULUS-ERROR\*

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The purpose of this paper is to discuss the "stimulus-error," to indicate something of its history (though limits of space will preclude more than a bare outline), to add something by way of definition (since definition has remained implicit and there are some who do not understand this term), to enquire, at the level of the scientific experiment, into the significance of the attitude which is thus styled an "error" (relying as much upon experimental observation and as little upon epistemological conviction as is possible), and to arrive, if may be, at an evaluation of the stimulus-error or stimulus-attitude in its relation to the psychology of the present day. This is not so large an order that it does not need filling. Some psychologists put out of court experiments that involve the stimulus-error; others refuse to see any 'error' at all and discount the works that stress this 'merely epistemological' distinction. And when we seek a sanction for the one view or the other, we are at a loss whither to turn, for the "stimulus-error," although it has a long history, has been left to make its way without any very formal introduction.

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\* The present paper is the outcome of a promise to deal specifically with the nature of the stimulus-error, especially with its relation to psychological measurement and psychophysics. Cf. the discussions of the present writer in *The logic of the normal law of error in mental measurement*, *Amer. J. Psychol.*, 1920, 31, 1-33, esp. 27ff.; and in *The control of attitude in psychophysical experiments*, *Psychol. Review*, 1920, 27, 440-452, esp. 447f., 449 and note.

*The Stimulus-Error*

Undoubtedly much of the confusion and disagreement has been brought about by the term itself: "stimulus-error." It implies something that is right and something that is wrong, defending one position and impugning another. It serves, and was intended, to throw two positions into contrast, to insist upon an important distinction that is often overlooked; yet does not stop with definition, but goes on to pass a judgment. In this dual function of the phrase there has been both an advantage and a disadvantage. To those who accept both the implied distinction and the explicit evaluation the notion has been exceedingly useful, for it has enabled them, not only to separate the methodological sheep from the goats, but also to dispense with the goats,—a telescoping of procedure that is convenient and economical. On the other hand, those psychologists who have staked their fortunes on the goats of stimulus are not to be reformed by being found in the way of the "stimulus-error." They simply deny the "error" and in so doing miss the more fundamental distinction between opposing positions that must be made out before judgment can be passed upon either. We ourselves must not be thus misled, whatever our ultimate judgment may be.

This implied opposition, which we must now bear clearly in mind, is the fundamental opposition in psychology—or between psychologies—of mental process and meaning, of content and object, of *Beschreibung* and *Kundgabe*. Titchener, who is responsible for the term "stimulus-error," puts the case thus:

"We are constantly confusing sensations with their stimuli, with their objects, with their meanings. Or rather—since the sensation of psychology has no object or meaning—we are constantly confusing logical abstraction with psychological analysis; we abstract a certain aspect of an object or meaning, and then treat this aspect as if it were a simple mental process, an element in the mental representation of the object or meaning. . . . We do not say, in ordinary conversation, that this visual sensation is lighter than that, but that this pair of gloves or this kind of grey paper is lighter than this other. We do not say that this complex of cutaneous or organic sensations is more intensive than that, but that this box or package is heavier than this other. We do not even say, as a rule, that this tonal quality is lower than that, but rather that this instrument is flat and must be tuned up to this other. Always in what we say there is a reference to the objects, to the meaning of the conscious complex. It is not the grey, pressure, tone, that we are thinking of; but the grey of leather or paper, the pressure of the box, the pitch of the violin. . . . What is more natural than to read the character of the stimuli, of the objects, into the 'sensations' with which certain aspects of the stimu-

lus or object are correlated? . . . This is what Fechner did. . . . [He] transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology."<sup>1</sup>

We commit the stimulus-error if we base our psychological reports upon objects rather than upon the mental material itself, or if, in the psycho-physical experiment, we make judgments of the stimulus and not judgments of sensation. At the more complex levels we may make a similar error, a 'meaning-error,' which consists of describing objects, reporting meanings, stating *Kundgabe*, instead of describing mental process or giving *Beschreibung*. We can not, however, in this paper, extend the discussion to include this complex level, but must content ourselves with the conviction that whatever applies in the controversy between judgment of stimulus and judgment of sensation, applies also to introspection and its rival, the statement of meaning. We may concede that the psychophysical experiment in its simplicity represents the ideal ultimate in the psychological experiment, where control of conditions and adequacy of observation are maximal; and that we should be glad to reduce all psychological observation to this degree of rigor at least. At any rate any extension of this discussion to the 'higher' processes must wait, for the history and application of the stimulus-error are at the level of psychophysics, and the interpretation of the "stimulus-error," up to which we are leading, shows most clearly here.

#### *The Quantity Objection and the Stimulus-Error*

It is not surprising that a psychophysics, which seeks to establish the relation between the mental and the physical, should emphasize the distinction between sensation and stimulus. What is surprising is that the opponents of psychophysics should have raised this very distinction for the confounding of psychophysics and should have claimed that the psycho-physical relationship (the logarithmic relation of the Weber-Fechner law) was an artifact created, not by the attempt of the psychophysicist to distinguish between sensation and

<sup>1</sup> E. B. Titchener, *Experimental Psychology*, II, i, 1905, p. xxvif.; cf. *Text-book of Psychology*, 1910, 202f. Titchener first uses the term stimulus-error = "R-error" in *Exper. Psychol.*, II, ii, 1905; see pp. lxiii, 198f., 203ff., 207, 219, 223, 230f., 262, 450. For Titchener's further use of the term, see *Text-book*, pp. 218, 350, 398 note, 522. J. v. Kries characterized the objectifying attitude as a "source of error": "Wenn man aber diese Quelle des Irrthums ausschliesst und möglichst an den objectiven, als Reiz dienenden, Vorgang gar nicht denkt . . . ;" *Vtjschr. f. wiss. Philos.*, 1882, 6, 275; and Titchener seems to refer to this discussion as a sanction for the term "stimulus-error."

stimulus, but by his confusion of the two. Yet such is the substance of the "quantity objection" to psychophysics, which had later to be met with the psychological sense-distance by Müller, Titchener, and others, who thus turned the tables and brought the argument for the distinction between mental and physical material to the support of a Fechnerian psychophysics. This was a long and tedious battle, and one might have expected that the resultant emphasis upon the two-fold nature of psychophysics would have determined the psychophysical universe for a time. On the contrary, however, the confusion between sensation and stimulus persisted. Cattell was fathering a psychology of the stimulus, and it was in the tradition of the work of Fullerton and Cattell that Urban did his experiments. Now that behaviorism has come into vogue, it is not apparent that we do not have two kinds of psychophysics—a psychophysics of process that gives, as Fechner wanted, the correlation between mental and physical data, and a psychophysics of behavior that seeks to identify response with its stimulus. That this psychophysics of stimulus-and-response needs also, if it is to be scientific, to take account of the error that has been called the "stimulus-error" is the thesis of the present paper; but the thesis must wait upon the perspective of the preface.

A clear recognition of the distinction between mind and body, between consciousness and objects, was the key-note of Fechner's position. There was for him at least this dualism in the universe, which may be regarded from one standpoint or the other. The case is not unlike, Fechner argued, the Ptolemaic and Copernican worlds. The geocentric and heliocentric solar systems are different systems, and we may at pleasure take either point of view that we choose. The worlds remain distinct. Or the matter is like a circle, which may be viewed from the inside or the outside. In the one case we see only concavity, in the other only convexity. Such a dualism can be resolved only by the law of relationship that holds between its two aspects, and, just as the relationship between concavity and convexity can be stated geometrically for the circle, so the logarithmic law resolves the dualism of mind and body. There is no doubt, therefore, that Fechnerian psychophysics stands or falls according to its success in distinguishing between measurements of mind and measurements of body, or between sensation and the object of sensation, the stimulus.<sup>2</sup>

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<sup>2</sup> G. Th. Fechner, *Elemente der Psychophysik*, esp. 1889, I, 1-12.



The vigorous opposition that developed to Fechner's psychophysics took its stand firmly upon the distinction between mind and body, but denied the possibility of a quantitative correlation between the two on the ground that mind was not possessed of magnitude and that mental measurement was an impossibility. This argument came to be known as the "quantity objection" and was the main source of opposition to quantitative psychology in the eighties and nineties of last century. Introspection, the objection runs, does not show that a sensation of great magnitude ever contains other sensations of lesser magnitude in the way that a heavy weight may [supposedly] be made up of a number of smaller weights. "Our feeling of pink," said James, "is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow-candle in itself."<sup>3</sup> "This sensation of 'gray,'" remarked Külpe, "is not two or three of that other sensation of 'gray.'"<sup>4</sup> "A blue surface," Ebbinghaus commented, "is something other than a green, but the latter has in itself, apart from memory of the colors, nothing of the doubleness or threefoldness of the green. . . . A low tone sounds different from a high tone, and in like manner a loud tone different from a soft."<sup>5</sup> In other words increase of magnitude in no sense means increase of complexity. A sensation is just itself no matter what its degree. The tone produced by many instruments in unison is not of itself composed of more units than is the tone from a single string, nor is the tone of many vibrations per second more complex than the tone of few vibrations. In this form the objection seems obvious enough. Sensational magnitude is certainly not multitude, and intense sensations are not integrated of more sensory stuff than are weak.<sup>6</sup> How then was psychophysics to defend itself?

Its immediate defense was a display of the factual material. Here were the experimental measurements. If they were not observations of the magnitudes of sensation, what were they?

<sup>3</sup> W. James, *Principles of Psychology*, 1890, I, 546.

<sup>4</sup> O. Külpe, *Outlines of Psychology*, tr. 1896, 45.

<sup>5</sup> H. Ebbinghaus, *Z. f. Psychol.*, 1890, 1, 323.

<sup>6</sup> Titchener mentions as raising the quantity objection: G. E. Müller in 1878, Exner in 1879, Stadler in 1880, Zeller in 1881, Boas and F. A. Müller in 1882, Stumpf in 1883, Tannery in 1884, Elsas in 1886, Grotenfelt in 1888, James, Münsterberg, and Ebbinghaus in 1890, Sully in 1892, Külpe in 1893, Wahle in 1894, Meinong in 1896, Höfler in 1897, and Lehman in 1902. See Titchener, *Exper. Psychol.*, II, ii, pp. xlviii-lxiii.

To this question the raisers of the "quantity objection" replied that psychophysicists had created an artificial mental magnitude by a confusion of the sensation with the stimulus, that is to say, they had committed in their experimental work the "stimulus-error." This was a serious charge against a discipline that depended for its existence upon a sharp distinction between the mental and the physical. Let us see how the accusers dared to raise it.

Von Kries put the matter clearly:

"An illusion is thus very easily brought about by the fact that one tends in general to estimate objective values (measurable in objective terms) according to the sensation. If one, however, excludes this source of error, and in so far as possible thinks not at all of the objective process serving as a stimulus, then one must necessarily admit that a quantitative relation does not exist between the different parts of an intensive series. This fact is most obvious to us when we do not attempt objectification as, *e.g.*, in pain. Whatever it is called, a pain exactly ten times as strong as another does not admit of such absolute statement."<sup>7</sup>

Ebbinghaus, somewhat later, was even more explicit:

"In general one designates the brightness of a flame or a surface as 10 or 12 times another brightness, and could just as easily, it appears, designate a loud tone as the double or treble of a soft tone. But what occurs here is no longer an immediate sensation or an immediate judgment of sensations, but depends upon the introduction of experiences. We can readily experience, and we do every day experience, the fact that the arousal of a brightness or a loudness depends upon a diversity of just those physical things or processes that in limited number call forth the impression of darker or softer. In order to have an impression of greater brightness for a surface, one can increase the number of gas-flames illuminating it; in order to strengthen a tone, one multiplies the instruments carrying it. Such experiences with respect to the causes of sensations we have always in immediate view, and we believe that we have the numerical characteristics that always attach to the one occurring without anything further in the other. It is psychologically difficult to get rid of them, just as it is difficult not immediately to see in a grass-green apple its sourness. But if one succeeds in the perfectly possible separation of the thought context, then it is clear that, as the bare visual impression of an apple has no sourness in it, similarly the bare impression of brightness does not consist of the multiplicity of candles upon which, of course, it frequently depends."<sup>8</sup>

We have already seen what fifteen years later, Titchener had to say in the same vein and how, although defending mental measurement, he makes the charge of the stimulus-error against Fechner. And there were many others.

<sup>7</sup> J. v. Kries, *loc. cit.*

<sup>8</sup> H. Ebbinghaus, *op. cit.*, 323f.; cf. *Grundzüge der Psychologie*, I, 1905, 71-79.

Exner put forward the general argument in 1879;<sup>9</sup> and Boas in 1882.<sup>10</sup> Tannery said in 1883: "It is the objective study of the excitation and its variations that leads to this definition of number that measures the sensation. At bottom it is by excitation that sensation is defined."<sup>11</sup> On epistemological grounds both F. A. Müller<sup>12</sup> and Meinong<sup>13</sup> concluded that mental magnitudes, unlike physical, were indivisible. And long before any of these, Brentano, the father of modern intentional psychology, had said: "If one measures, as Fechner did, the intensities of colors, tones, *etc.*, then one is measuring the intensities of physical phenomena. The color is not the seeing, the tone is not the hearing, the warmth is not the sensing of warmth."<sup>14</sup>

Nevertheless this still seems a surprising charge to bring against Fechnerian psychophysics. If the fundamental task of psychophysics is the discovery of the relationship between the hitherto unrelated body and mind, is it not astonishing that psychophysics should have confused the two, the two whose very separateness was the *raison d'être* of psychophysics? Yet the critics stuck to the point and were at pains to show the readiness with which these incommensurables did duty, the one for the other. Ward, pointing out that the psychophysical limen expressed in terms of stimulus was physical quantity, concluded: "There is no trespass harder to avoid than that across the lines dividing the subjective and objective aspects, and none more disastrous to the offender."<sup>15</sup> Other writers urged the same point, and Külpe even brought the prevalence of objectification into an experimental study.<sup>16</sup> It is no wonder then that objectification was thought of as a source of error and that Titchener coined for it the term "stimulus-error."

### *The Answer to the Quantity Objection*

The fundamental and final answer to the quantity objection was Weber's law:  $S = k \log R$ . In so far as the relationship had been observed, no amount of explaining could explain it entirely away. It might be that the function was not exactly

<sup>9</sup> S. Exner, Hermann's *Handbuch der Physiologie*, 1879, II, ii, 242.

<sup>10</sup> F. Boas, *Pfüger's Arch.*, 1882, 28, 568f.

<sup>11</sup> J. Tannery in J. Delboeuf, *Éléments de psychophysique*, 1883, 138.

<sup>12</sup> F. A. Müller, *Das Axiom der Psychophysik*, 1882, 46-56.

<sup>13</sup> A. Meinong, *Z. f. Psychol.*, 1896, 11, 81-133, esp. 96ff.

<sup>14</sup> F. Brentano, *Psychologie vom empirischen Standpunkte*, 1874, I, 91.

<sup>15</sup> J. Ward, *Mind*, 1876, O. S. 1, 460.

<sup>16</sup> O. Külpe, Ueber die Objectivirung und Subjectivirung von Sinesseindrücken, *Philos. Stud.*, 1902, 19, 508-556.

logarithmic or that it held only within certain limits; it might not be certain just what was the nature of S, or of R; but the unescapable fact was that there were an S and an R, which were covariant, and which were not identical since the mode of variation of the one was not the mode of variation of the other. To charge the stimulus-error and say that S was contaminated by R was not enough, since the confusion of S with R was not enough to explain the discovery of this difference in variation. The psychophysicists, therefore, had the stronger position, and had only to show where the difference actually lay. There seem to be five ways of accounting for the difference and thus of establishing psychophysics.

1. Systematically one may argue for a physiological interpretation of Weber's law, as Müller did.<sup>17</sup> Excitation varies somewhat as does the logarithm of the stimulus. No one doubts that excitation may have magnitude, and thus the quantity objection is met. Moreover the logarithmic relation between physical dependents is not unknown.<sup>18</sup> Excitation, however, does not happen to be open to immediate observation, so we must observe its correlate sensation. We deal therefore with stimulus and sensation, which we must keep apart, avoiding the stimulus-error; and we escape from the formal objection that sensation does not have magnitude by making it a mere qualitative indicator of excitation which must have magnitude.

2. Wundt's psychological interpretation of Weber's law meets the quantity objection by the introduction of *Merklichkeitsgrade*. Sensations do not have magnitude, but if they did the matter would be irrelevant to psychophysics. It is apperception that gives a quantitative aspect to mind; there are degrees of noticeableness to sensations or to the differences between them.<sup>19</sup> The sensation scarlet is not more than the sensation pink, but is more noticeable than the pink; and the difference between a scarlet and a pale pink is more noticeable than the difference between the scarlet and a rose. To introspection it is just as obvious that apperception has degree as it is obvious that sensation has not, and it is between these

<sup>17</sup> G. E. Müller, *Zur Grundlegung der Psychophysik*, 1878, 224-403.

<sup>18</sup> Müller, *loc. cit.*; Ward, *op. cit.*, 452-466; Titchener, *op. cit.*, II, ii, 66f. The autocatalytic theory is more recent: T. B. Robertson, *Monist*, 1909, 19, 372ff., 384f.

<sup>19</sup> Wundt's theory passed through successive stages and no brief statement does it justice. For summary and discussion, see Titchener, *Exper. Psychol.*, II, ii, pp. lviff., lxxivf., lxxxff., 69f.; for summary and genesis, p. lxxxii. note.

*Merklichkeitsgrade* and the physical values of the stimulus that the logarithmic law holds.

3. What was Fechner's answer to the quantity objection? To deny the stimulus-error, which the quantity objection implies. In the *Revision* he wrote:

"One must take care not to try to count relations that exist in the physical realm between physical units as existing within the mental province because they can be grasped only by the mind; for in so doing one loses the ground for distinction between the two provinces. Even the physical, within which the relations exist, must be grasped by our minds in order to exist for us and to be spoken about. Yet we discriminate on the basis of this community between outer and inner phenomena as between two provinces, and have to distinguish properties as belonging in the one or the other, not merged with each other or interchangeable, as might sometimes seem to be the case under a philosophical point of view. At any rate psychophysics takes this point of view and thus avoids confusion. Whenever something like a relation, a change, a difference, a unity, or a fusion, *etc.*, appears as characterizing the physical or psychical world, it is abstracted from the province of the one or the other, or it is counted into the one or the other province; it may occur just as readily in psychology as in natural science. Hence it is perfectly possible that the pitches should be represented in one province by something in the other province without our identifying the two. The relations of periodicity between vibrations, which occur as the psychophysical representatives of melodic and harmonic sensations, are the most obvious; the one is something very different from the other."<sup>20</sup>

In other words it is all in our point of view. We can judge the stimuli or we can judge the sensations; and, according as we do the one or the other, we constitute for ourselves the physical or the mental world. It is nonsense to assume that, because we make judgments of physical phenomena in building up natural science, this natural science is a science of judgments and therefore mental. The two are distinct, and the discovery of the difference that is summarized by Weber's law attests the distinction.

To the writer of this paper it seems that Fechner's argument, turned a different way, becomes at least as invincible as any of the other ways out of the difficulty. The trick for escaping the force of the quantity objection, when directed against mental phenomena, is to turn it upon the stimuli themselves. Suppose sensations of weight do not under observation exhibit magnitude; what of the physical weights themselves? To physical observation ten grams is *a* weight and one gram is *a* weight; it is only in common sense, which is assuredly not physics, that ten grams is ten one-gram weights. Because physical phenomena, like mental, are referable to

<sup>20</sup> Fechner, *Revision der Hauptpunkte der Psychophysik*, 1882, 5f.



objects, is no excuse for reading the objects into them. Physical weight is as little the number of objects in the scale-pan as mental weight is the number of weights in the hand. The physical quantity is just as simple and unitary as the mental, and if sensation lacks magnitude so must stimulus. We can hardly, however, deny measurement to physics, and it thus appears that the quantity objection is not valid either against the measurement of sensation.<sup>21</sup>

4. Undoubtedly the most general way of meeting the quantity objection while saving mental measurement is by the substitution of the sense-distance for the sensation magnitude. Historically this conception dates from Delboeuf's *contraste sensible*. It is not necessarily incompatible with any of the foregoing accounts of mental measurement and is endorsed essentially by Wundt, Boas, Stumpf, Ebbinghaus, James, Meinong, Höfler, Stout, and G. E. Müller.<sup>22</sup> It is the basis of Titchener's quantitative psychology, where it finds its clearest exposition.<sup>23</sup>

This position holds that sensations, although they do not possess magnitude, may lie within a continuum, and that, although we can form no quantitative estimate of any sensation, we can nevertheless estimate the relative degree of separateness of two sensations within the continuum. Sensations are simply themselves and are not summed of various numbers of increments; the distances between these sensations, however, do vary and can be estimated in amount. The simplest case of mental measurement occurs when, for a series of three sensations, A, B, and C, occurring in a continuum, we estimate the sense-distance AB as equal to the sense-distance BC. Here we have measurement, for we have laid off the unit  $AB=BC$  twice in the distance AC, and it is the correlation of such estimated sense-distances with the corresponding values of stimulus that gives Weber's law.

<sup>21</sup> The more thorough exposition would show that magnitude and measurement are systematic matters and are not found immediately at the observational level of science. The confusion is not unlike that of the systematic 'sensation' with the observational 'attribute:' cf. Sensation and system, *Am. J. Psychol.*, 1915, 26, 258-267, where Titchener makes this point. On the other hand, it is hardly fair to physics to say: "No sensation is a sum of sensation-parts or of sense-increments; no sensation is a measurable magnitude. Fechner has transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology:" Titchener, *Exper. Psychol.*, II, i, p. xxvii.

<sup>22</sup> Cf. Titchener, *op. cit.*, II, ii, p. cxxxiii.

<sup>23</sup> Titchener, *op. cit.*, II, i, pp. xxi-xxvii; ii, pp. cxvi-cxlv.

5. All the foregoing modes of meeting the quantity objection are successful without sacrificing the possibility of mental measurement; the fifth mode of defense consists in joining the enemy. We can give up the measurement of mind, substituting the measurement of sensitivity or of capacity-for-discrimination. Fullerton and Cattell give us our orientation here.

They declare, in the first place, that both sensation magnitudes and sense-distances are undiscoverable:

"If an observer can, in fact, estimate quantitative amounts of difference in sensation, apart from association with known quantitative differences in the stimuli, a relation between mental and physical intensity can be determined. The writers, however, agree in finding that they cannot estimate such quantitative differences in sensation in a satisfactory manner. We can indeed say when one weight seems approximately double another, but this is doubtless because we have often lifted first one volume, and then two, and the like. But we cannot say when one sound seems twice as loud, or one day twice as hot as another. We have made experiments to see how nearly different observers would agree in adjusting one shade of light midway between two others, and have found hesitation in coming to a decision and great divergence of opinion. Most men will think that a just king is happier than a tyrant, but few will agree with Plato in considering him 729 times as happy."<sup>24</sup>

What is left? The observed *stimuli*<sup>25</sup> and errors of observation incurred in observing the stimuli.<sup>26</sup> There is no constant just noticeable difference nor threshold.<sup>27</sup> We have only errors of observation as we fail to observe an actual difference in the stimuli or, less often, observe a difference that is not there. These errors can be treated under the ordinary calculus of probabilities and follow the normal law of error.<sup>28</sup> The amount of the average error is always determinable and it increases with the magnitude of the stimulus.<sup>29</sup> It is the law of the dependence of the average error of observation upon the magnitude of the stimulus that Weber's law seeks to state, although the mathematical form of Weber's law is actually incorrect.<sup>30</sup>

Such a quantitative psychology of error is of necessity a psychology of capacity,<sup>31</sup>—of the capacity of the organism to

<sup>24</sup> G. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences*, 1892, 20.

<sup>25</sup> Fullerton and Cattell, *op. cit.*, 9ff., 20, 153, etc.

<sup>26</sup> Fullerton and Cattell, 14ff.; Cattell, *Am. J. Psychol.*, 1893, 5, 287ff.

<sup>27</sup> F and C, 11, 150; Cattell, 288f.

<sup>28</sup> F and C, 12ff.; Cattell, 285ff.

<sup>29</sup> F and C, 23ff., 153f.; Cattell, 290ff.

<sup>30</sup> F and C, 24ff., 152.

<sup>31</sup> Cf. Titchener, *Exper. Psychol.*, II, ii, p. cxxxiv note.

respond correctly to stimuli. This point of view has since developed into the statistics of mental tests and of behaviorism, which is a psychology of stimulus and response. It is a point of view for which there is no stimulus-error since quantitative judgments can be made only of the stimulus, and it is one that touches other sciences very closely since it concerns itself with observation, the method of every science.<sup>32</sup>

### *The Two Psychologies*

Thus it becomes evident that the answers to the quantity objection have divided along the traditional cleft in psychology. We have not only a psychology of datum and a psychology of capacity, but we have quantitative psychologies of datum and of capacity.<sup>33</sup>

The quantitative psychology of datum ["the given"] insists upon a truly mental measurement. When further it correlates these mental measurements it is the true psychophysics. It may answer the quantity objection in any of the first four of the five foregoing ways, because any one of those four insists upon or explains the existence of mental quanta. It is a part of the larger psychology that is variously called structural or introspective, the psychology of process or of *Beschreibung*.

The quantitative psychology of capacity admits the quantity objection and denies—or at least ignores—mental quanta. This psychology sees no distinctively mental measurement, but undertakes the physical measurement of bodily response as a function of the physical quantities of the stimulus. There is no sharp epistemological line discernible between this sort of measurement and other physical measurement, and it thus meets the requirement of modern behaviorism that psychology interpenetrate physical science without sensible demarcation.<sup>34</sup> The psychology of capacity is also the psychology of mental tests<sup>35</sup> and of Urban's psychophysical experiments.<sup>36</sup> These

<sup>32</sup> F and C, 9; Cattell, 285.

<sup>33</sup> Cf. O. Külpe, *Vorlesungen über Psychologie*, 1920, 5ff.; Titchener, *Am. J. Psychol.*, 1921, 32, 108-120.

<sup>34</sup> J. B. Watson, *Psychol. Rev.*, 1913, 20, 177; *J. Philos., etc.*, 1913, 10, 427; *Psychology*, 1920, vii.

<sup>35</sup> See note 31 above.

<sup>36</sup> The present writer has already had occasion to refer to F. M. Urban's position: E. G. Boring, *Am. J. Psychol.*, 1920, 31, 27f., esp. note 77; *Psychol. Rev.*, 1920, 27, 446f. It is well to repeat that no reference is intended here to Urban's later position, which involves an acceptance of the epistemology of Mach and Avenarius; see Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.*, 1913, 30, 113-152, esp. 113, 124f., with notes; cf. also *Am. J. Psychol.*, 1913, 24, 274.

latter seem strange mates, but the influence of Cattell is apparent in both. If the germ of the tests was in Galton, nevertheless it was Cattell, in the interests of the psychology of individual differences, who planted it in American soil where it has brought forth fruit abundantly. Urban's origin is less apparent. It is trivial to remark that he began his psychophysics in the laboratory that Cattell had founded years before. The evidence is internal and not explicit, but the seeker after information can find it.<sup>37</sup> In the broad, we may add, the psychology of capacity may become a functional psychology and deal, when it is experimental, with *meanings* and *Kundgaben* instead of mental processes and *Beschreibung*.

As a term the "stimulus-error" is the property of the psychology of datum. It characterizes the attitude of the psychology of capacity as being concerned observationally with the stimulus and admonishes against it. Of course the psychology of capacity ignores this admonition, for in making observations of the physical stimulus it is simply fulfilling its self-appointed task. The "stimulus-error" is no valid charge against this psychology, nor does it raise within it a question of right and wrong. No more can be expected of either psychology than that it hold to its premises.

In practice there is a difficulty, however. No matter how distinct the two psychologies may be, psychologists can scarcely hold strictly to the one or to the other. The psychologist of the datum can not be expected to attack a new perception without recourse to judgments of stimulus or of meaning,<sup>38</sup> and statements of meanings moreover may themselves become the object of psychological investigation.<sup>39</sup> As to whether the psychology of capacity might remain wholly faithful to the

<sup>37</sup> On psychophysical judgments and random events, compare Fullerton and Cattell, *op. cit.*, 12ff., 23ff., with Urban, *The Application of Statistical Methods to the Problems of Psychophysics*, 1908, 17f. On the meaning of the just perceptible difference, compare F and C, 11, and Cattell, *op. cit.*, 288f., with Urban, *Stat. Meth.*, 70. On mental measurement compare F and C, 20, 152f., and Cattell, 293, with Urban's ignoring of the issue in *Stat. Meth.* [Or could he have thought that Titchener had settled it for the sense-distance? Cf. *Psychol. Bull.*, 1912, 9, 245.] On judgment as directed upon stimulus, compare F and C, 20, and Cattell, 293, with Urban, *Stat. Meth.*, 5, 17, *Psychol. Rev.*, 1910, 17, 27ff. On recording degrees of assurance, compare F and C, 11, 151, with Urban, *Stat. Meth.*, 5ff. On the relation of psychophysics to physics, compare F and C, 151, Cattell, 285, with Urban, *Psychol. Rev.*, 17, 243f.

<sup>38</sup> Cf., e.g., L. B. Hoisington, On the non-visual perception of the length of lifted rods, *Am. J. Psychol.*, 1920, 31, 114-146.

<sup>39</sup> Cf., e.g., H. P. Weld, Meaning and process as distinguished by the reaction method, *Titchener Commemorative Volume*, 1917, 181-208.

stimulus it is not so easy to say. This attitude is more natural and it is possible to complete entire experiments without once trespassing upon the realm of mental process. On the other hand, the functional psychologies for all they have to say of the inadequacies of structural psychology, seem usually unable to complete the systematic mental picture alone, and a recent system of behaviorism has drawn unhesitatingly upon the psychology that it seeks to supplant.<sup>40</sup> In general what is fundamental to the one can not be ignored by the other, and on this account the writer of this paper would urge the attention of the psychologist of capacity to the "stimulus-error."

#### *The Effect of the Stimulus-Error*

If we are now to urge upon the psychology of capacity the avoidance of the stimulus-error, it is a fair demand that we state first the probable penalty that is incurred by a failure to accept our advice. Here we can not stand upon the epistemological ground that psychology observes mental processes and not stimuli, and that judgments of stimulus are therefore *a priori* inadmissible. This historical warning against the stimulus-error does not apply to the psychology of capacity which protests against a scientific dualism and deals by preference with stimulus and response. What we have to show is rather that the stimulus-error works against the establishment of the univocal correlations between stimulus and response that a psychology of capacity demands, that it interferes with the prediction of the response for a given stimulus. Here the ground is broadly scientific: we are dealing with the constancy of experimental conditions and the reproducibility of results.

When we go frankly to the literature, however, asking just what in numerical terms may be the effect in mental measurement of allowing judgments of the stimulus or of instituting them, we meet at first disappointment.

The psychology of datum is set to avoid, rather than to measure, the stimulus-error. It tells us where the stimulus-error is most insidious, *viz.*, in judgments of supraliminal sense distances.<sup>41</sup> We may have trouble with the limens; we are almost sure to have it in comparing large sense-distances. Sometimes a special technique is necessary to avoid the error. In Martius' experiment on the apparent size of objects at different distances from the eye, all the stimulus habits for the estimation of the size of objects in everyday life are

<sup>40</sup> Watson, *Psychology, from the Standpoint of a Behaviorist*, 1919.

<sup>41</sup> Cf., e.g., Titchener, *Textbook*, 218.



appealed to, and a special method is required "in removing the initial tendency of the observers to reflect on the actual size of the comparison rod in relation to the standard rod."<sup>42</sup> In Angell's experiment on intensive distances between sounds one would expect concrete reference to the stimulus to enter much less readily; on the contrary, however, the observers tend to judge the height of fall or the angle of fall of the stimulus, and to neglect the mental datum.<sup>43</sup> For this error special remedies are presented. Müller urges that the amount of intensive difference be taken as the *Kohärenzgrad*, the *Leichtigkeit des Kollektivaufgefasstwerdens* of the two sounds.<sup>44</sup> Titchener suggests letting the observer blunder into the stimulus-error and then rescuing him by individualized treatment, his protocols in hand.<sup>45</sup> These experiments are striking cases, but even the psychologists who have no special measures of reform to offer cry out against the evils of judgments based upon secondary criteria, upon associates of the processes judged, or upon surrogate processes.<sup>46</sup> A surrogate, they complain, can even render incommensurables artificially commensurate, as seems to be the case when the intensities of weights and noises are compared in terms of *Spannungsempfindungen*.<sup>47</sup>

Now it is not likely that there could be so much smoke without some fire; yet we are still at a loss to estimate the amount of danger. It would be reasonable for these psychologists of datum to seek to avoid stimulus for no other reason than that they are interested solely in "mind," but it is not to be supposed that the matter would have been taken so seriously had the quantitative results, and Weber's law which is dependent upon them, been unaffected by the kind of judgment given. Indeed this belief came to the surface when Grotenfelt, in defense of Weber's law, accused Merkel of the stimulus-error and inclined to the belief that results

<sup>42</sup> G. Martius, Ueber die scheinbare Grösse der Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder, *Philos. Stud.*, 1889, 5, 601-617, esp. 605f.; cf. Titchener, *Exper. Psychol.* II, ii, pp. 262f.

<sup>43</sup> F. Angell, Untersuchungen über die Schätzung von Schallintensitäten nach der Methode der mittleren Abstufungen, *Philos. Stud.*, 1891, 7, 414-468, esp. 438.

<sup>44</sup> G. E. Müller, *Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik*, 1904, 237f.

<sup>45</sup> Titchener, *Exper. Psychol.*, II, ii, p. 198; cf. also pp. 203f., 230.

<sup>46</sup> Cf. Fechner, *Elemente*, II, 318ff.; H. Neiglick, *Philos. Stud.*, 1888, 4, 41; Angell, *op. cit.*, 438; W. Ament, *ibid.*, 1900, 16, 173; G. E. Müller, *op. cit.*, 241; J. Fröbes, *Z. f. Psychol.*, 1904, 36, 259.

<sup>47</sup> H. Münsterberg, *Beiträge zur experimentellen Psychologie*, III, 1900, 56-122, esp. 98ff.

that followed Merkel's law might possibly indicate that they were based upon judgments of stimulus.<sup>48</sup> The data that we wish, however,—the comparison in quantitative terms of results of judgments of process—are, in the earlier history of psychophysics, lacking.

(The psychologists of capacity, we may note in passing, are not to be asked for this comparison. Cattell and Fullerton denied the possibility of mental measurement. They can not therefore be asked for its comparison with any form of physical measurement.)

On the basis of recent literature, however, there is something to be said, in answering this question, for the case of lifted weights, and very much to be said for the case of the limen of dual cutaneous impression. We may mention the lifted weights at once and reserve the two-point limen for the next section.

Friedländer undertook a comparative study of lifting weights under different *Einstellungen*.<sup>49</sup> He employed a "*G-Einstellung*" in which the attention was directed upon the lifted object (*Gegenstand*, hence "*G*"), and an "*A-Einstellung*" in which the object was abstracted from (hence "*A*") and the attention directed upon the sensory aspect of the experience (*Druck-, Spannungs-, Kraftempfindungen*).<sup>50</sup> Here we should expect to find the results we are seeking, for the *G-Einstellung* is the stimulus-attitude, the attitude demanded by a psychology of capacity and called the "stimulus-error" by the psychology of datum; and the *A-Einstellung* in its various forms is the process-attitude which avoids the "stimulus-error." There is not the least doubt that the two attitudes give different results. "Differential sensitivity on the whole is somewhat finer for the G-series" for a standard weight of 500 g., Friedländer tells us, but a standard of 1200 g. may give a finer discrimination under the *A-Einstellung*. The data unfortunately are for one observer only. They are based on too few cases,—41 series after the practice-effect was presumably constant. The observer did not always succeed in maintaining the required attitude, for the stimulus-attitude was difficult for him (!) and he sought to make his finer discriminations under the *A-Einstellung*. The resultant psychometric functions are not smooth ogives; one just barely misses inversion in its central

<sup>48</sup> A. Grotenfelt, *Das Webersche Gesetz und die psychische Relativität*, 1888, 111f.; cf. Titchener, *Exper. Psychol.*, II, ii, pp. lxxviii f., 219.

<sup>49</sup> H. Friedländer, *Die Wahrnehmung der Schwere*, *Z. f. Psychol.*, 1920, 83, 129-210, esp. 187-193.

<sup>50</sup> Pp. 133ff.

portion.<sup>51</sup> We are not yet ready, then, to generalize as to the exact effect of attending to the stimulus in making psychophysical judgments. All we can say is that there is an effect, that a shift in the observational attitude alters the numerical results significantly. We shall not dare, therefore, if we wish to predict response from stimulus, to leave attitude out of account.

My colleague, Professor Fernberger, has recently completed similar experiments in the Clark Laboratory. He had three observers, and extended his series through many fractions to take account of progressive practice and to give an adequate number of cases. His resultant ogives are smooth and regular, and his procedure seemed calculated to yield all that could be desired methodically. He gets differences for the different attitudes,—more striking differences in some cases than Friedländer's. More than this I can not say in advance of the publication of his results. Perhaps in his final analysis he will discover a generalization, which is not apparent to casual inspection of the functions and constants. The results indicate unquestionably that an alteration of attitude by instruction may result in an alteration of the psychometric functions, which is significant in the mathematical sense of being many times its probable error, but which for a given observer is unfortunately quite unpredictable. Attitude may be very important even when we can not say just why.

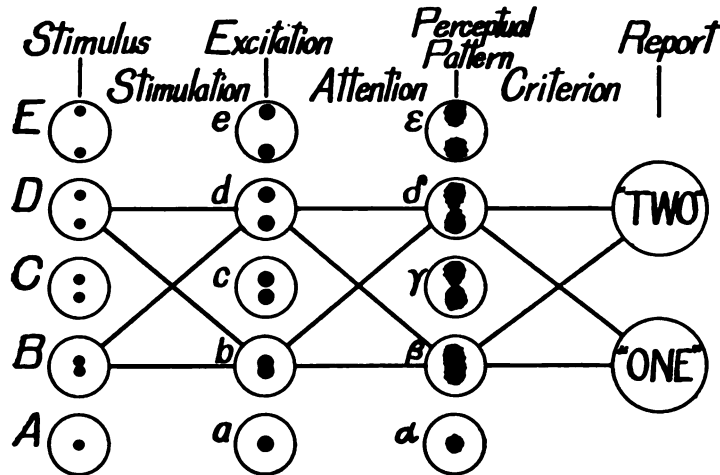
Fortunately the case of the two-point limen is less mysterious.

#### *The Stimulus-Error as Equivocal Correlation*

The limen of dual impression upon the skin furnishes the case for which we are looking. We know, not only that judgments of stimulus may here make a difference in the quantitative results, but we know further how great this difference may be and something of its conditions. We are in a position, moreover, to generalize from these facts with some assurance and to assert that the effect of the "stimulus-error,"

<sup>51</sup> In fact it is not even clear that Friedländer's cautious generalization is not in part an artifact. If we compute the data of Table 10 by Urban's procedure for the *Konstanzmethode*, we get:

	A-Einstellung	G-Einstellung
Av. measure of precision (h)	0.0138	0.0147
Interval of uncertainty (grams)	35.39	37.13
Point of subjective equality (grams)	500.8	509.9
There is not much difference in precision or discrimination by this method. The striking difference is in the effect of attitude on the point of subjective equality.		



from the point of view of a psychology of capacity, is—under similar conditions, at least—to render the correlations between stimulus and response equivocal and thus to jeopardize the rigor of conclusion that science demands.

We may proceed to the point by reference to the visual schema of the figure reproduced herewith. The diagram is intended merely to assist in the analysis of the factors involved and not as an actual picture of neural or psychophysical fact.

One deals in determining the two-point limen with a series of stimuli, *A, B, C, D, E*, pairs of stimulus-points at different separations, with perhaps a single point, *A*, at the extreme of the series. From the work of numerous investigators<sup>52</sup> we know that there is a similar series of perceptual patterns, *α, β, γ, δ, ε*, which passes, with approximate regularity, from a sharp point to a blunt point, to an oval, to an elongated oval, to a double-paddle, to a dumb-bell, to two separated points. We may best think of these perceptual patterns as the process material of the psychology of datum, but they exist also for the psychology of capacity as inscrutable "middle terms." Intermediate between stimulus and process we are accustomed to assume some sort of excitatory process, *a, b, c, d, e*. Of

<sup>52</sup> G. A. Tawney, *Psychol. Rev.*, 1895, 2, 585-593; V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes*, 1898, 6; M. Foucault, *L'illusion paradoxale et le seuil de Weber*, 1910, 122-145; E. J. Gates, *Am. J. Psychol.*, 1915, 26, 152-157; and numerous other references cited in these articles.



these there must also be a series, and knowing little about them, we may picture them as somewhat like the perceptual processes. Finally there is the verbal report which is generally limited experimentally to the words *One* and *Two*. It seems, moreover, that we keep within the limits of scientific good sense if we say that stimulus, excitation, mental process, and report form a dependent series, and that each is the resultant of the preceding.

If now we choose for special consideration the stimuli *B* and *D*, we may note that they most frequently, perhaps, give rise to the dependent series '*B-b-β-One*' and '*D-d-δ-Two*,' but we must note further that cross-connections are possible.

Stimulus gives rise to excitation by way of stimulation. Here variation may tend either in the direction of fusion or of separation. On the forearm, for example, the angular shifting of stimulus toward the longitudinal axis may result in fusion and *D* may give rise to *b*; if the rotation is toward the transverse axis, *d* may follow upon *B*.<sup>53</sup> Location as well as angular orientation also yields these differences. Near the elbow *D* gives *b*, say; a little further down *B* may give *d*.<sup>54</sup> It is also possible that the chance impingement of the stimulus-points upon multiply innervated spots in the skin gives rise to multiple excitation (*B* to *d*) and that the stimulation of less complexly innervated spots may lead to a simpler excitation (*D* to *b*).<sup>55</sup>

At the next phase of our series excitation culminates in perceptual pattern under the selective action of attention. These terms sound strange of course to the psychologist of capacity, but we may ask him to accept them or to find substitutes for them. They stand for factors that affect the correlations with which he is working, and he must take scientific account of them, whatever he calls them.

It is experimentally demonstrable that under inattention potential *Twos* become *Ones*; one stimulating point or the other catches the attention and thus withdraws it, as it were, from the other.<sup>56</sup> The converse occurs in the well-known *Vexirfehler*, the paradoxical judgment, where two points close together or even a single point give a perception of duality. Henri and Tawney referred this phenomenon in part to attention, and its dependence upon certain suggestive factors

<sup>53</sup> Cf. Tawney, *Philos. Stud.*, 1897, 13, 170; cf. Boring, *Quart. J. Exp. Physiol.*, 1916, 10, 23.

<sup>54</sup> Cf. Henri, *op. cit.*, 26f.; cf. Boring, *loc. cit.*

<sup>55</sup> Henri, *op. cit.*, 64ff.; Boring, *op. cit.*, 89-93, esp. 93.

<sup>56</sup> Cf. A. Brückner, *Z. f. Psychol.*, 1901, 26, 54f., 60; Boring, *op. cit.*, 88f.



seems to indicate that it is of this order.<sup>57</sup> Griesbach and others supposed that fatigue reduced the limen,<sup>58</sup> but Friedline has shown that the effect of fatigue is operative only among the perceptual forms at the lower end of the series, say  $\alpha$  and  $\beta$ . Here fatigue acts in the direction of fusion and may perhaps be another factor that operates within the attentive phase.<sup>59</sup> Thus the perceptual pattern is not wholly dependent upon conditions at the periphery, the mode and place and nature of the stimulation. Certain more central factors also come into play and justify us in adding to the cross-connections  $B-d$  and  $D-b$  the connections  $b-\delta$  and  $d-\beta$ .

Finally now the perceptual pattern issues in a judgment in accordance with criteria of judgment that have been established. The available factor here lies in the setting up and the preservation of these criteria. Left to himself there is no guarantee of what the subject will do, as the studies of Titchener,<sup>60</sup> deLaski,<sup>61</sup> and Friedline<sup>62</sup> all show. Perhaps the most normal criteria for sophisticated adult subjects would be such that the perceptual  $\delta$  of our chart would lead to the judgment *Two* and the perceptual  $\beta$  to the judgment *One*. It is a reasonable inference that McDougall's savages in Torres Straits, under the competitive incentive to do well, assumed such criteria that even our  $\beta$  would have led to the judgment *Two* for plainly  $\beta$  is larger than the pattern which a single point most often gives.<sup>63</sup> An overzealous subject can interpret perceptual patterns lying very low in the series as meaning the presence of two points upon the skin; in fact, as Friedline has shown, he can do amazingly well under the influence of both practice and incentive, provided he is not fatigued.<sup>64</sup> On the other hand the sophisticated subjects of the laboratory, and, it may be, therefore the Englishmen, whom McDougall compared with the savages, tend to define *Two* by reference

<sup>57</sup> Henri and Tawney, *Philos. Stud.*, 1895, 11, 394-405, esp. 403ff.; Tawney, *ibid.*, 1897, 13, 186-198; Henri, *Raumwahrnehmungen*, 61-66.

<sup>58</sup> H. Griesbach, *Arch. f. Hygiene*, 1895, 24, 124ff.; but see the summary in C. L. Friedline, The discrimination of cutaneous patterns below the two-point limen, *Am. J. Psychol.*, 1918, 29, 415-418.

<sup>59</sup> Friedline, *op. cit.*, 411f., 418f.

<sup>60</sup> Titchener, On ethnological tests of sensation, etc., *Proc. Am. Philos. Soc.*, 1916, 55, 206-215.

<sup>61</sup> E. deLaski, On perceptive forms below the level of the two-point limen, *Am. J. Psychol.*, 1916, 27, 569-571.

<sup>62</sup> Friedline, *op. cit.*, 405-415, esp. 405f., 408f., 411ff.

<sup>63</sup> W. McDougall, *Rep. Cambridge Anthropol. Expedition to Torres Straits*, 1903, II, 189-193; cf. Titchener, *op. cit.*

<sup>64</sup> Friedline, *op. cit.*, 408f., 414f.

to a perceptual pattern higher in the scale.<sup>68</sup> We should hardly have been bold enough to have predicted this result; to the writer, however, it seems reasonable enough now that it has been pointed out. Is it not to be expected that the savage would try to "do well" by discriminating as finely as possible and that the sophisticated person would try to "do well" by discriminating as accurately and consistently as possible, though less finely? In any case the point is that the acceptance of a criterion is an unavoidable experimental condition in determinations of cutaneous spatial sensitivity, and that the criterion must therefore be controlled, since when uncontrolled it gives uncertain significance to the verbal responses *Two* and *One*. In fact it appears that the apparent limen for cutaneous duality may be very much more than quartered by a variation of criterion, and it seems further that the conflicting results in the literature with respect to the effect of fatigue and the effect of practice upon the limen are to be explained in this way.

This leads to a conclusion. If only the end-terms of stimulus and response are controlled a univocal one-to-one correlation between stimulus and response is not possible. In the terms of the diagram, both the stimuli *D* and *B* may condition the response *Two*. There are eight paths leading to *Two*, four from *D* and four from *B*. If we consider the other factors of the schema, the situation is enormously complicated. There are over a hundred modes of connection from *A*, *B*, *C*, *D*, and *E*. to *Two*. Certainly the actual possibilities must be legion. At this level of work the best we can do is to remain in the dark and to deal with relative frequencies, yet relative frequencies do not yield the predictive correlation that science demands. The only way to get out of the dark would be to study the effect of stimulation, of attention, and of criterion by taking hold of these dependent series at their intermediate points, thus providing ourselves with a more complete knowledge and control of the entire psychophysical situation. Now the psychologist of capacity habitually controls stimulation, the various adjustments of the stimulus to the sense-organ, but the psychologist of datum also controls by instruction both attention and criterion. He does not, to be sure, reach the ideal of 100% certainty in the prediction of the response to

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<sup>68</sup> McDougall, *op. cit.*, 192 and note; Titchener, *op. cit.*, 211.

a given stimulus, but he is able greatly to increase the precision of these stimulus-and-response correlations. On the other hand, the failure to control the attitudinal factor implied in the acceptance of a criterion, and the attentional factor, again and again results perforce in an equivocal determination of these responses, which is nothing more nor less than a "stimulus-error."

In the psychology of capacity, then, the danger of the "stimulus-error" reduces to the danger that judgments of stimulus will prove scientifically equivocal. Experimentation will show when they are and when they are not. When they are equivocal, the problem is soluble by refinement in the control of conditions. The modern technique for the control of attention and attitude is a method that satisfies scientific standards of accuracy of prediction better than any available substitute. To shut our eyes to this technique in the absence of a substitute would be to refuse to accept scientific methods that have already yielded practical results. Certainly if the psychologist of capacity is to be a successful experimenter, joining hands with the biologist and physicist, he must in some way take account of all the means of experimental control that have been demonstrated as essential to the securing of accuracy.

### Summary

Scientific psychology in its inception assumed a distinction between mind and matter and the separate existence of observable mental data and observable physical data. Fechner's psychophysics sought to measure the mental data and to establish their correlation with related physical data. The opponents of this point of view raised the *quantity objection*, arguing that mind is not possessed of magnitude and is therefore not measurable. Most of these objectors were attacking only the quantitative status of psychology and seeking to establish it as an essentially qualitative, but mental, science. Other objectors preferred, however, to keep psychology quantitative by conceiving of it as physical, as the psychology of the *capacity* of the organism for response to stimulus. The older psychology met the quantity objection by showing that the nonexistence of mental magnitude does not preclude mental measurement, and then sought to protect itself against incursions of the physical observational attitude of the psychology

of capacity by styling that attitude the *stimulus-error*. The implication would be that a psychology of capacity does not need to avoid the stimulus-error, but rather should cultivate it. The thesis of this paper is, however, that recent researches have shown that the observational attitude which is directed upon the stimulus—the attitude of the stimulus-error—may sometimes lead to equivocal correlations of stimulus and response which, because equivocal, are unscientific. In the case of the limen of dual impression upon the skin, for example, a psychology of capacity must make use of introspective data if it is to attain its own ideals.

# THE ILLUSORY PERCEPTION OF MOVEMENT ON THE SKIN<sup>1</sup>

By ANNA KELLMAN WHITCHURCH

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The object of the present investigation was to determine the elementary conditions of the perception of cutaneous movement. Wertheimer<sup>2</sup> and his successors have found such conditions for the perception of movement in the field of vision; and Dimmick<sup>3</sup> has shown a specific existential correlate of the perception of visual movement, namely, the grey flash. Our problem, then, is to find out whether the perception of cutaneous movement can be aroused by successive stimulation, and in that case what is its existential correlate.

Benussi and others<sup>4</sup> have already done much work upon the arousal of cutaneous movement by successive stimuli operating at long distances apart. Benussi stimulated at distances ranging from 4 cm. to the distance between the fingers

<sup>1</sup> From the Psychological Laboratory of Cornell University.

<sup>2</sup> M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych.*, 61, 1912, 161ff.

<sup>3</sup> F. L. Dimmick, An Experimental Study of Visual Movement and the Phi Phenomenon, this JOURNAL, 31, 1920, 317ff.

<sup>4</sup> V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. u. d. VI Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59. Cf. also H. E. Burtt, Tactual Illusions of Movement, *Jour. Exp. Psych.*, 2, 1917, 371.



on the two outstretched arms. Burtt gives reports from stimulations 8 to 16 cm. apart, and his apparatus allowed distances from 2 to 18 cm. These distances, however, favor the arousal of visual associations; and minimal distances are for that and for other reasons to be preferred. Incidental reports by von Frey and Metzner,<sup>5</sup> apparently overlooked by later experimenters, prove that such distances may be employed.

Von Frey and Metzner have given the only reports of work on the skin directly comparable with that of Wertheimer in vision. They were trying to find the best interval in which to distinguish as different the responses of two adjacent pressure spots successively stimulated. They used as stimuli hairs or blunt needles which rested on the skin for 1/9 sec. The intervals between stimulations were 1/9, 1/6, 1/3, 1, 4/3, and 2 sec. Near the end of their paper this statement appears: "When two points are stimulated, the difference of the two stimuli is either recognized as such, or (especially with the shorter intervals) is interpreted as a shift (*Verschiebung*) of the stimulus, as a brushing (*Streichen*) of it over the skin." The only numerical data given show that, with an interval of 4/3 second, 13 out of 29 stimulations aroused, in some form, the perception of movement.

#### *Experimental Procedure*

The present work was begun in the summer of 1920. The four O's who served were: Miss C. Braddock (B), graduate scholar in psychology; Dr. K. M. Dallenbach (D), assistant professor of psychology; Dr. L. B. Hoisington (H), assistant professor of psychology; Mrs. A. K. Whitchurch (W), graduate scholar in psychology.<sup>6</sup>

The skin on the left fore-arm of the O's was tested with von Frey hairs of various tension-values until a hair was found which could be felt upon the normal skin, but was too weak to arouse subcutaneous sensations when the skin was lightly etherized. The tension-value of the hair finally selected was 7.9 gr./mm. Two hairs were cut to this standard. They measured in length 40 and 42 mm. respectively.

Five groups of pressure-spots, ranging from 6 to 9 in number, on every O's arm were isolated and stained, so that the same spots could be tested every day. During this preliminary testing one spot was stimulated only once in an hour. The stimulating hair was held 1 mm. above the pressure-spot, and

<sup>5</sup> M. von Frey and R. Metzner, *Die Raumschwelle der Haut bei Successivreizung*, *Zeit. f. Psych.*, 29 1902, 161ff.

<sup>6</sup> When W observed, B acted as experimenter.

after the "Ready, now" signal was touched to the spot. The *O*'s were asked to report on the intensity, quality, and extensity of the sensation. Two reports were taken when the spots were stimulated singly, and two when one spot was compared with another. The introspections were worked over, and the spots most alike were selected and again reported upon, until the first numbers chosen narrowed down to 3 or 4 in a group. Finally we were able to select two adjacent spots in every group which the *O*'s reported as like the spots in every other group. The selected spots were then permanently tattooed with a tiny glass tube just under the hair. The mark was distinct and lasting. The distances between the spots in the various groups were as follows (in mm.): B (1) 2.5, (2) 3, (3) 2.5, (4) 3, (5) 4; D (1) 5, (2) 3.6, (3) 3.2, (4) 3.8, (5) 3; H (1) 4.7, (2) 3.4, (3) 4.7, (4) 3.1, (5) 2.4; and W (1) 2.8, (2) 5.5, (3) 3.2, (4) 2.1, (5) 3.8.

#### *Manual Experiments*

*O* sat at a low table with his arm in a plaster cast. The adjacent hairs in the five groups were stimulated in turn. The duration of the stimulus was  $250 \pm 25\sigma$ . The intervals, marked by a silent pendulum, were 750, 1000, and 1250 $\sigma$ .

*O* worked under two sets of instructions. The 'process' instruction was: "I shall stimulate the skin on your arm. Describe the cutaneous perception aroused in purely psychological terms. The stimulus will be repeated so that you may complete your description." The 'meaning' instruction was: "I shall stimulate the skin on your arm. Characterize the cutaneous perception aroused, fully, in any commonsense terms you wish to use. The stimulus will be repeated so that you may complete your report."

The manual experimenting continued until every *O* had given 50 reports under the 'meaning' and 50 under the 'process' instruction with the 750 and the 1250 intervals. With the 1000 interval, every *O* made 100 reports under each set.

#### *Results of Manual Experiments*

The reports fell under headings like those which have been obtained in the work on vision: (1) full movement, in which the stimulus moved from the 1st to the 2nd spot stimulated; (2) bimembral movement, in which the 1st and 2nd stimulus each moved; (3) unimembral movement, in which either the 1st or the 2nd stimulus moved; (4) intramembral movement, in which the stimulus at one or both spots moved

within the area of stimulation; (5) pure phi, in which the movement had no qualitative, intensive, or extensive beginning and ending, but was a uniform 'brushing' over the cutaneous surface.

Table A sums up the results of this preliminary work. The columns headed M show the percentage of reports under the 'meaning' instruction; those headed P the percentage of reports under the 'process' instruction. The intervals are given in sigma.

The reports under the two instructions are closely parallel. Long before the completion of the series the O's had settled into a stable manner of reporting.

The 750 interval is the best of the three for arousing the perception of full movement. Even with this interval, however, the percentages are very low. Since optimal conditions had not been found, and since the lowest interval was the best, we began a new series of experiments with shorter intervals. Henceforth we applied the stimuli by means of apparatus.

#### *Mechanical Stimulation: Series I*

The apparatus was an adaptation of Benussi's kinohapt.<sup>7</sup> The intervals and durations of the stimuli were controlled by a Leipsic time-sense apparatus.

Both cutaneous and 'subcutaneous'<sup>8</sup> stimuli were employed. The cutaneous stimuli were two horse-hairs whose tension-value was 4.85 gr./mm., and which measured in length 10 and 9.8 mm. respectively. The subcutaneous stimuli were coarser hairs whose tension-value was 10.25 gr./mm.; they measured 3 and 2.85 cm. respectively. The duration of the stimuli was 150σ. The intervals of this series were 100, 250, 500, 750, and 1000σ. The adjacent spots in two groups on every O's arm were now stimulated exclusively.

The cutaneous (C) and subcutaneous (S) stimuli were used alternately as follows: (1) C 100, 12 M-reports on spots *a-b*, 12 P-reports on spots *a'-b'*; (2) S 250, 12 M-reports on spots *a-b*, 12 P-reports on spots *a'-b'*; (3) C 500, 12 M-reports on spots *a-b*, 12 P-reports on spots *a'-b'*; and so forth. At the end of the ten groups which this order necessitated, the experiments were continued by reversing the order. Thus after no. 10 came (11) S 100, 12 P-reports on spots *a-b*, 12 M-reports on spots *a'-b'*. The practice-effect was thus evenly

<sup>7</sup> For a description of the kinohapt, see V. Benussi, *Arch. f. d. ges. Psych.*, 29, 1913, 385.

<sup>8</sup> We use this shorthand term to designate the stimuli which affected both cutaneous and subcutaneous end-organs.

TABLE A  
MANUAL EXPERIMENTS: PERCENTAGES

		750		1000		1250	
		M	P	M	P	M	P
Full Movement	B	18	14	18	19	4	2
	D	4	4	10	16	14	2
	H	48	30	27	34	0	0
	W	14	24	23	24	2	2
	Av.	21	18	19.5	23.3	5	1.5
	M.V.	13.5	9	5.5	5.8	4.5	.8
Bimembral Movement	B	4	2	5	0	2	2
	D	0	0	0	0	0	6
	H	4	0	10	8	2	0
	W	2	10	13	15	6	10
	Av.	2.5	3	7	5.8	2.5	4.5
	M.V.	1.5	3.5	4.5	5.8	1.8	3.5
Unimembral Movement	B	12	26	9	21	42	38
	D	12	8	26	11	30	20
	H	34	54	32	34	48	42
	W	56	38	38	30	36	48
	Av.	28.5	31.5	26.3	24	39	37
	M.V.	16.5	14.5	8.8	8	6	8.5
Intramembral Movement	B	22	12	17	7	12	12
	D	14	0	3	6	0	4
	H	2	0	13	4	20	14
	W	10	6	8	6	22	6
	Av.	12	4.5	10.3	5.8	13.5	9
	M.V.	6	4.5	4.8	.9	7.5	4
Phi	B	0	2	1	1	4	0
	D	6	0	10	1	2	2
	H	2	2	0	1	0	0
	W	12	10	3	6	0	2
	Av.	5	3.5	3.5	2.3	1.5	1
	M.V.	4	3.3	3.3	1.9	1.5	1
No Movement	B	44	44	50	52	36	46
	D	64	88	51	66	54	66
	H	10	14	18	19	30	44
	W	6	12	15	19	34	32
	Av.	31	39.5	33.5	39	38.5	47
	M.V.	23	26.5	17	20	7.8	9.5

distributed. Several minutes elapsed between the stimulations of the same spot. At the end of every 12 reports *O* took his arm from the cast for a time, in order that there might be no fatigue. Usually *O* made reports on one group of experi-

ments in an hour, and never on more than two. Every O made 24 reports under the 'meaning' and 24 under the 'process' instruction for every one of the intervals employed.

### Results of Series I

The results of the first series with mechanical stimulation are summarised in Tables B I and B II. The columns M and P show the number of reports of the different types of phenomena given in each group of 24 experiments.

With both cutaneous and subcutaneous stimuli the 100 interval gives the greatest number of full movements. Next to the full movement in frequency comes the unimembral movement. The number of full movements is too small to indicate optimal conditions, and another series was accordingly planned.

The 'process' and 'meaning' reports run closely parallel, as is shown by the averages of the groups.

TABLE B—I  
CUTANEOUS MECHANICAL STIMULATION: SERIES I: ABSOLUTE NUMBERS

	100		250		500		750		1000	
	M	P	M	P	M	P	M	P	M	P
Full Movement	B	9	6	3	4	0	0			
	D	8	10	3	0	0	0			
	H	6	8	7	5	1	0			
	W	13	14	0	0	0	0			
	Av.	9	9.5	3.3	2.3	.3				
	M.V.	2	2.3	1.9	2.3	.4				
Bimembral Movement	B	0	0	0	1	0	3	2	0	1
	D	0	0	0	0	0	0	0	2	2
	H	7	5	6	6	9	7	0	0	0
	W	8	5	5	7	8	8	2	1	0
	Av.	3.8	2.5	2.8	3.5	4.3	4.5	1	.3	.8
	M.V.	3.8	2.5	2.8	3	4.3	3	1	.4	.8
Unimembral Movement	B	5	8	6	4	10	8	5	6	4
	D	12	12	14	9	5	1	4	3	3
	H	10	11	10	11	8	9	9	10	9
	W	3	5	13	15	14	14	7	8	7
	Av.	7.5	9	10.8	9.8	9.3	8	6.3	6.8	5.8
	M.V.	3.5	2.5	2.8	3.3	2.8	3.5	1.8	2.3	2.2
Intramembral Movement	B			1		3	5	13	7	9
	D			5		0	0	0	0	1
	H			0		4	2	5	3	5
	W			2		0	0	3	2	3
	Av.			2		1.8	1.8	5.3	3	4.5
	M.V.			1.5		1.8	1.8	3.8	2	2.5
No Movement	B	9	8	14	15	11	8	4	11	10
	D	3	2	2	15	19	23	20	21	18
	H	1	0	1	2	2	6	10	11	10
	W	0	0	4	2	2	2	12	13	14
	Av.	3.3	2.5	5.3	8.5	8.5	9.8	11.5	14	13
	M.V.	2.9	2.8	4.8	7.7	6.6	6.6	4.5	3.5	3

With the 100 interval, B reported phi in 1 M and 2 P; D in 1 M.



TABLE B—II

SUBCUTANEOUS MECHANICAL STIMULATION: SERIES I: ABSOLUTE NUMBERS

		100		250		500		750		1000	
		M	P	M	P	M	P	M	P	M	P
Full Movement	B	10	13	2	3						
	D	14	10	18	11						
	H	15	13	7	7						
	W	15	15	11	10						
	Av.	13.5	12.8	9.5	7.8						
	M.V.	1.8	1.4	5	2.8						
Bimembral Movement	B	0	1	0	0	3	4	2	0	1	0
	D	2	3	6	8	2	0	2	0	0	0
	H	4	0	4	5	0	1	1	1	0	0
	W	4	6	3	5	6	9	4	2	0	0
	Av.	2.5	2.5	3.3	4.5	2.8	3.5	2.3	.8	.3	
	M.V.	1.5	2	1.8	2.3	1.8	3	.9	.8	.4	
Unimembral Movement	B	5	3	12	11	7	4	8	9	0	4
	D	2	4	0	1	12	20	7	6	2	0
	H	4	8	10	10	10	9	8	11	9	8
	W	5	3	10	8	9	7	13	15	7	4
	Av.	4	4.5	8	7.5	9.5	10	9	10.3	4.5	4
	M.V.	1	1.8	3.3	3.5	1.5	5	2	2.8	3.5	2
Intramembral Movement	B	0	0	0	0	2	3	6	5	3	4
	D	0	2	0	0	2	0	0	0	1	5
	H	0	0	2	1	5	8	5	4	4	5
	W	0	0	0	0	2	1	2	2	12	13
	Av.		.5	.5	.3	2.8	3	3.3	2.8	5	6.8
	M.V.		.8	.8	.8	1	2.3	2.3	1.8	3.5	3.2
No Movement	B	9	7	10	10	12	13	8	10	20	16
	D	5	5	0	4	8	4	15	18	21	19
	H	1	3	1	1	9	6	10	8	11	11
	W	0	0	0	1	7	7	5	5	5	7
	Av.	3.8	3.8	2.8	4	9	7.5	9.5	10.3	14.3	13.3
	M.V.	3.3	2.3	3.6	3	1.5	2.8	3	3.9	6.3	4.3

With the 100 interval, D reported phi in 1 M.

*Series II*

The intervals chosen for the next series were 25, 50, 75, and 100 $\sigma$ . The arrangement of the groups and the procedure were as before.

The results are summarised in Tables C I and C II. The 100 $\sigma$  interval gives the greatest number of full movements with cutaneous stimulation, and the 75 $\sigma$  interval with subcutaneous stimulation. Again the 'process' and 'meaning' reports parallel each other.

A group of 24 experiments with each stimulus in each of the intervals 125 and 150 $\sigma$  was made as a part of this series with O's H and W. Full movements were reported in the following cases: C 125, H 9M and 8P, W 8M and 6P; C 150, H 5M and 4P, W 4M and 4P; S 125, H 6M and 4P, W 5M and 3P; S 150, H 6M and 6P, W 5M and 4P.

TABLE C—I  
 CUTANEOUS MECHANICAL STIMULATION: SERIES II: ABSOLUTE NUMBERS

		25		50		75		100	
		M	P	M	P	M	P	M	P
Full Movement	B	0	0	19	23	17	16	14	19
	D	5	1	9	6	7	8	11	9
	H	2	6	10	4	11	11	14	15
	W	0	0	10	13	14	15	16	14
	Av.	1.8	1.8	12	11.5	12.3	12.5	13.8	14.3
	M.V.	1.8	2.1	3.5	6.5	3.3	3	1.4	2.8
Bimembral Movement	B			0	0	0	0	0	0
	D			0	0	0	0	0	0
	H			4	4	2	3	4	3
	W			2	0	4	3	4	5
	Av.			1.5	1	1.5	1.5	2	2
	M.V.			1.5	1.5	1.5	1.5	2	2
Unimembral Movement	B	1	0	0	0	0	3	2	2
	D	12	8	12	14	11	6	10	9
	H	5	5	9	11	7	7	6	6
	W	1	1	3	4	6	6	4	5
	Av.	4.8	3.5	6	7.3	6	5.5	5.5	5.5
	M.V.	3.8	3	4.5	5.3	3	1.3	2.5	2
Intramembral Movement	B	3	4	3	0	2	0	2	0
	D	0	0	0	0	0	0	0	0
	H	0	0	0	0	0	1	0	0
	W	2	0	0	0	0	0	0	0
	Av.	1.3	1	.8		.5	.3	.5	
	M.V.	1.3	1.5	1.1		.8	.4	.8	
No Movement	B	20	20	1	1	5	5	6	3
	D	7	14	2	4	6	10	3	6
	H	17	13	1	5	4	2	0	0
	W	21	23	9	7	0	0	0	0
	Av.	16.3	17.5	3.3	4.3	3.8	4.3	2.3	2.3
	M.V.	4.3	4	2.9	1.8	1.9	3.3	2.3	2.3

With the 25 interval, D reported phi in 1 P; with the 50 interval, B reported it in 1 M, and D in 1 M.

### *Types of Perception*

The reports arising from the cutaneous and subcutaneous stimulations are so nearly alike that we shall for the most part confine our discussion to the former and shall mention the 'subcutaneous' reports only where differences occur. The reports under the 'meaning' and 'process' instructions will not be treated separately. They will be grouped together in the following discussions, and throughout 'meaning' will be dealt with first.

(1) *Full Movement*.—In all cases of full movement, the stimuli are felt as two touches with movement between them from *a* to *b*. The type appearing most often is that of the dumb-bell pattern. The stimulus touches, lifts, and moves in a narrow line to a new position. In 'process' the first impression is a round or oval bit of extent. The impression

TABLE C—II

SUBCUTANEOUS MECHANICAL STIMULATION: SERIES II: ABSOLUTE NUMBERS

		25		50		75		100	
		M	P	M	P	M	P	M	P
Full Movement	B	1	5	5	2	11	14	15	17
	D	7	6	6	1	18	12	11	15
	H	0	0	3	3	16	15	10	10
	W	0	0	2	0	19	18	17	15
	Av. M.V.	2.5	2.8	4.5	1.5	16.5	14.8	13.3	14.3
Bimembral Movement	B			0	0	0	0	0	0
	D			0	0	0	0	0	0
	H			0	2	0	2	3	1
	W			0	0	1	2	1	0
	Av. M.V.				.5	.3	1	1	.3
Unimembral Movement	B	0	0	1	6	1	5	0	0
	D	11	13	16	21	0	0	9	2
	H	2	1	9	9	6	7	11	13
	W	4	2	4	4	4	4	6	9
	Av. M.V.	4.3	4.5	7.5	10.5	2.8	4	6.5	6
Intramembral Movement	B	7	0	2	3	2	1	1	2
	D	0	0	0	0	1	0	0	0
	H	0	1	3	1	0	0	0	0
	W	0	0	0	0	0	0	0	0
	Av. M.V.	1.8	.3	1.3	1	.8	.3	.3	.5
No Movement	B	16	19	16	13	10	4	8	5
	D	6	5	2	2	5	12	3	7
	H	22	22	9	9	2	0	0	0
	W	20	22	18	20	0	0	0	0
	Av. M.V.	16	17	11.3	11	4.3	4	2.8	3
		5	6	5.8	5.5	3.3	4	3.9	3

With the 100 interval, D reported phi in 1 M.

extends in time into another oval or circular bit of experience like the first. The following reports are typical:

H (C 100 M) The hair touched lightly and began moving at once. As it moved it pressed more lightly and almost lifted off. Then, as it moved on, it pressed harder, and came down fairly lightly at the end. It moved faster at the beginning than it did in the middle.

H (C 100 P) The process began limited in extent. It increased in one direction to fairly extended, with slight contraction ending in a little spread of extent. It began as neutral pressure, becoming increasingly like contact until it was almost pure contact, with a rapid shift to pure neutral pressure. It was fairly weak, diminishing in intensity during the extent of the process, and becoming more intense at the end. The whole was very short.

The next type is much like the dumb-bell pattern, save that the movement does not start from the center of the first touch, but rather begins at the edge. In 'process' the first impression is described as static. Then there follows a growth from the edge.

H (C 100 M) A hair touched and rested; and then something started to move from the edge of the first touch and moved a little way, rested, and lifted off.

H (C 100 P) The process was limited and static for a bit; then growth began from the edge and became fairly extended. At first it was a dull neutral pressure, quickly shifting to a brighter pressure, and becoming neutral at the end. It was fairly weak, and dropped to weak. It was of short duration.

D (C 100 P) The process began as a contact fused with a wiry prick, immediately followed by a vague, diffuse, large pressure sensation localized differently.

In a third type the touches at beginning and end are of the same size as the moving touch between. There is no narrowing during the movement. In 'process' the terminals are marked by differences in quality and intensity, but not by a spread of extent. The first may be a pressure rather intense, merging into a spreading that is less intense, but of continuous extent, and ending in another intense pressure of the same extent.

W (C 100 M) A touch moved smoothly across my arm. It pressed harder at the beginning and ending than it pressed in the middle of its course. It was the same breadth all the way.

W (C 100 P) The process began, limited in extent, and spread in time, becoming weak during the spread but not contracting, and increasing in intensity as it became more extended. It began as good contact, and remained contact throughout. There was no change in extent at the terminals. The whole was short.

D (C 100 P) The process was neutral pressure, the area of which seemed to expand to another neutral pressure.

B (C 100 P) The process was a contact changing in intensity along a line.

The *O*'s describe the extension of the process in various other ways. It is called a 'flowing,' an 'extending in time,' a 'diffusing in time.' At first the word 'flow' was frequently used, but during the later reports it does not appear. Two *O*'s have said that 'flow' does not describe truly the depth of the sensation. Even when the intensity is weak, the growth seems to have the feel of something deeper than 'flow.' 'Ooze' has sometimes been used. H usually described the process as 'growth'; B and W used the term 'spread' most frequently; D spoke of 'extension and diffusion,' 'sensory continuum in time,' 'progression in time.'

With cutaneous stimuli the terminals, with few exceptions, are contact or neutral pressure. The same terms usually describe the subcutaneous experience; occasionally this is called 'drag' in the reports of H and W. In a few cases the subcutaneous stimuli produced a 'thicker' feel in the spread than did the cutaneous. In nearly all cases, except

those just mentioned, the reports for the two stimuli are exactly alike.

(2) *Full Movement in an Arc*.—Occasionally the O's reported a full movement in the shape of an arc. The first touch became larger in some one direction, and at the coming of the second it seemed to be pulled round to that position. In 'process' this type is described as a pressure becoming extended irregularly, and spreading around one side to another impression.

D (C 75 M) A touch went around in an arc to a new position.

B (C 100 M) The hair moved on my arm in the form of an arc, and rested in another place.

H (C 100 P) The process began limited and spread in extent out around one side, becoming narrower and ending in a slightly greater extent. It was neutral pressure, then contact with a shift to neutral pressure. It was fairly weak, decreasing in intensity during the extent, and increasing to fairly weak at the end. It was of short duration.

B (C 100 P) A contact diffused in a half circle and fused with another contact.

No O reported the arc movement more than five times in all the experiments performed. It occurred only when full movements were reported. Unimembral and bimembral movements did not take the arc form.

(3) *Bimembral Movement*.—In the bimembral movement the stimuli are felt as two touches which move, but which have a spatial and temporal interval between them. In nearly all cases *a* moves toward *b*, and *b* moves away from *a* to a resting place. Occasionally *b* touches and then moves on and off without resting. In 'process' the experience is described as two pressures or contacts, separated spatially and temporally, both diffusing in time.

B (C 500 M) A hair touched me and moved a bit. It lifted off, and came down moving into a new place.

H (C 100 P) The first process began very limited and grew in extent to slightly extended. It was a neutral pressure quality fading out, and it was very short. The second process was limited and shrank in time to very limited. It was neutral pressure with a hint of contact at the beginning, and it was weak and short.

D (C 100 P) Two neutral pressures occurred successively in time and were localized differently in space. The first became diffused in time, and the second likewise.

(4) *Unimembral Movement*.—In the unimembral movement, sometimes the first stimulus moved and the second was stationary, but more often the second was the moving member. Stimulus *a* touched and was gone; *b* touched while moving away from *a* to a new position. In 'process' one of the impressions was said to spread or extend in time.



H (C 100 M) A hair touched me fairly lightly and lifted off. It touched again in another place, moving a short distance. It rested and lifted off.

W (C 100 P) First there was a contact limited and very weak. Near it in space was a contact spreading in time to a larger extent and becoming more intense during the spread.

H (C 100 P) The process began limited, and grew slightly in extent; it was dull neutral pressure becoming pure pressure in the extent. It was fairly weak, with the intensity dropping off. It was very short. The second process was very limited neutral pressure, and was fairly weak and short.

There were other reports more rarely given. Sometimes only one stimulus was felt, and it moved to a stopping place, or began as static and moved off. In 'process' such experiences were called "a spreading contact growing more extended and more intense as it spread," or "a contact which spread and faded out."

(5) *Intramembral Movement*.—The intramembral movement is "the feeling of movement as if the hair were held in an unsteady hand," or "a wiggling of the hair against the skin." In 'process' the experience is described as "a slight shift of the intensive pattern in time."

D (C 250 M) There were two touches successive in time and localized differently in space. The first quivered as if held in an unsteady hand.

B (C 500 P) There were two contacts differently localized and successive in time. The second had changes of intensity in time; otherwise it was just two.

W (C 1000 P) The first process was a contact which fluctuated in intensity in the touched area. The second process was pure contact, very limited and short.

H (C 100 P) The first process was very limited and rather a brightish pressure of moderate intensity and very short. The second process was a fairly limited neutral pressure quality. There was a shift in time of the intensive patterns from moderate to fairly weak. The whole was short.

(6) *Intramembral plus Unimembral Movement*.—In the manual experiments and the first series of the mechanical stimulations there were reported some cases of combined perceptions of intramembral and unimembral movement. Sometimes one stimulus gave the perception of intramembral movement, and the other that of unimembral movement. Occasionally one stimulus would begin its movement as intramembral, and then extend into unimembral movement. These reports do not appear in the last series, in which the reports of unimembral and intramembral movements became uniform and clear-cut, with no hint of confusion or addition.

(7) *Phi Movement*.—The phi movement occurs infrequently. A typical 'meaning' report is: "It felt as if a moving some-

thing flicked against my arm." In this perception we do not have the feeling of two touches, or of one touch moving to a new position. In 'process' there were no distinct terminal experiences; the pressure is called "a weak, spreading contact, a bit more intense in its middle portion." Most of the pure phi-reports occurred in the manual experiments, and came early in the series.

(8) *Backward Movement*.—In the manual experiments and in the first series with the mechanical stimulations there were a few reports of unimembral and bimembral movement in which the stimulus seemed to move backward. That is, one impression was felt; and the second, instead of moving away from it to a new position, would be felt some distance away moving toward the first. H and W reported this perception three or four times in the manual experiments, but not afterwards. B reported it twice in the manual experiments; four times in Series I with the mechanical stimulations, and not at all in Series II. D reported backward movement three times in the manual experiments; in Series I of the mechanical stimulations he gave no such reports; in Series II he reported it 24 times (6.25% of all the reports in that series).<sup>9</sup>

(9) *No Movement: Two Points Distinct*.—In nearly all cases in which no-movement was reported, the perception was of two points touched successively in time and having spatial separation. Sometimes the one impression was more diffuse than the other, or differed from it slightly in quality; usually the two were reported as alike. This type of perception occurred in all experiments with intervals above 50°.

(10) *No Movement: Conjunctive*.—A report made frequently with the 25° intervals belongs to the no-movement group, though it indicates something more than merely two touches which did not move. The second stimulation was added to the first, or placed beside it. Sometimes the second grew larger after it had appeared, and again it came as a diffuse, ill-defined touch. In 'process' one contact or pressure had added to it another, and there was little separation of the two in space and time.

D (C 25 M) There were two light touches which came separately, but whose areas overlapped.

H (C 25 P) The first process was very limited. It was a weak contact and very short. Beside it came another process of pressure

<sup>9</sup> D's observation of backward movement evidently became a matter of habituation. Very occasionally in this series he reported triple pressure; as, for instance, pressure at *a* followed by a pressure at *b* which moved back toward *a* and then ended with increased intensity.

quality, with a fringe having in it a hint of contact. It was of very weak intensity and very short.

H (C 25 P) The process was very limited and was added to. It became fairly limited in one dimension. It was of neutral pressure quality with a fringe of contact pressure. It was fairly weak. The whole process was fairly extended and fairly weak.

(11) *No Movement: Superpositive.*—Occasionally the two stimulations seemed to be successive in time but to fall upon the same spot.

W (C 25 M) There were two touches, first one and then another beside it covering it. The two were almost successive in time.

H (C 25 P) The first process was very limited, and while it was still present another limited process covered it. The second was more extended, but there was perfect fusion of the two. Both were good neutral pressure quality, of weak intensity. The first was very, very short. The second was slightly longer.

D (C 25 P) The first process seemed obscured by the second. I was conscious of the first, but it was absorbed by the second, which was big and which gradually became bigger.

It will be noted that whereas B, H and W have a large number of reports falling into the no-movement group in the 25 $\sigma$  interval, D has but few.<sup>10</sup>

#### *Distance of Movement*

Under the 'process' instruction the O's never reported how far the stimulus moved in full movement, or how far apart were the members in part movement. In 'meaning' the perceptive distance in the larger intervals, as occasionally reported, was always greater than it was in reality, and varied according to the interval. With the 1000 $\sigma$  interval it was reported as 1.5 to 2 cm. As the intervals shortened, the distance decreased to 4 or 5 mm. at the 100 $\sigma$  interval, and at the 25 $\sigma$  interval the stimulations were side by side.

#### *Rate of Movement*

Rate of movement, like distance, was reported only under the 'meaning' set. Sometimes during the manual experiments the O's said that the movement went "quick as a flash" or "rapidly." Sometimes in the full movement reports one part of the movement was said to move more quickly than another. In bimembral movements, the O's occasionally said that "the second went quicker than the first." Aside from instances where one part of a movement could be compared with another in the same perception, the O was uncertain of the rate, and hesitated to report it. During the mechanical

<sup>10</sup> See footnote 9.

stimulations the reports of rate dropped out. If questioned about it, the O's said: "I am not sure," or "I can't report on it."

#### *Stimulations of Long and Short Duration*

After the series with the duration of the stimulus at 150 $\sigma$ , four other series (2C and 2S) of 24 experiments each were performed with H and W. In these series the 75 and 100 intervals were used, and the durations were 75 and 200 $\sigma$ . No full movements were reported by W with duration at 200 $\sigma$ ; H gave such reports in 6M and 4P at C 100. With duration at 75 $\sigma$  with C 100, H reported full movement in 14M and 17P, and W in 14M and 15P. These reports are almost the same in number and content as those with the duration at 150 $\sigma$ . With C 75, H had no full movements and W had 5M and 8P. With S 75 neither H nor W reported full movement. With S 100, H had 8M and 2P full movements, and W had 8M and 8P. Since the intervals longer and shorter than 150 proved to be not so good as 150, and in only one case compared favorably with it, no farther experiments were performed with them.

#### *Experiments Upon Non-Adjacent Spots*

A short series of 24 experiments with each stimulus was performed upon non-adjacent spots. The spots *a-c* and *a'-c'* were used. H and W again served as O's. The stimulations were made with the 75 and 100 $\sigma$  intervals. The duration was 150 $\sigma$ . H reported no movement during any of the experiments. W in S 75 had 8M and 12P unimembral movements. The movement always occurred with the second member. With C 100, W reported 12M and 14P unimembral movements, also at the second member. The movements were very short. There was just a hint that the second stimulus slid a very short distance into place. In 'process' the second stimulus had a very slight diffusion in time.

#### *Cutaneous Versus Visual Movement*

Dimmick under his best conditions obtained full movement with 'process' instructions in 90 to 93% of his reports, and with 'meaning' instructions in 81 to 91%. We under the most favorable conditions obtained on the average with C only 13.75 (57%) M-reports and 14.25 (59%) P-reports out of a possible 24; and with S, 16 (67%) M and 14.75 (61%) P. It seems from a comparison of these figures that the cutaneous perception of movement is less fundamental and



compulsory than the visual. There was, it is true, a difference in the plan of the two investigations, since Dimmick's stimulus *a* in his regular series always bore a fixed spatial relation to his stimulus *b*; whereas the spatial relations of the spots stimulated by us varied from group to group. Our stimulus *b* might lie above or below *a*, to right or to left of *a*. It is difficult to say how far this difference influenced the results; but it seems from what indications we have that the influence was slight. (1) A special C-series made with H, in which the same two spots *a-b* were stimulated in the same order in successive observations to the point of exhaustion, yielded no increased percentage of full movements. (2) The conditions of our mechanical stimulation approximated those of Dimmick's experiments. After the first two or three stimulations of two spots at the beginning of a group of twelve experiments, *O* knew when the second stimulation came that it was in the same place as before, and so learned to expect it at that place. H, who observed in Dimmick's work, said: "I was not conscious of expecting the second stimulation in the place where it came, but when it did come I had the feeling that it was where it ought to be and where it had been before." H further remarked that the expectancy was "the same sort of thing as in Dimmick's experiments." W, who also observed with Dimmick, verifies these reports.

Only one *O*, B, reported associated visual images, and she only in the early stages of practice. H and W declared that visual images did not appear; and D when questioned said that he had them occasionally, but that they were never a determining factor in his perceptions.

The aberrant types of perception, the arc-movement and the backward movement, seem to be conditioned upon some directional emphasis within the tactual spread. Ordinarily the spread of sensation is roughly circular or oval. If, however, some lateral segment of *a*'s spread is unusually intensive, the arc movement necessarily follows; and if *a*'s spread is uniformly weak, and *b*'s is strongest over a segment that points toward *a*, the perception of backward movement follows. We have no means of knowing how these local emphases arise, but the *O*'s reports show that they appeared and formed the basis of judgment. There is, of course, nothing parallel in Dimmick's results.<sup>11</sup>

<sup>11</sup> Dimmick tried to get backward movement by artificial emphasis and failed: *op. cit.*, 331.



*Conclusions*

(1) We have succeeded in synthetizing the perception of cutaneous movement, from two separate and successive stimulations, under conditions that parallel Wertheimer's synthesis of visual movement.

(2) The optimal conditions for the arousal of the perception are: with cutaneous stimuli, a duration of 150 $\sigma$  and an interval of 100 $\sigma$ ; with stimuli that also affect the subcutaneous organs, a duration of 150 $\sigma$  and an interval of 75 $\sigma$ .

(3) (a) The most important conditions of the perception, under our conditions, are the interval elapsing between the successive stimuli and the adjacency of the spots stimulated.

(b) The duration of 150 $\sigma$  is best for arousing the perception of full movement; the number of full movements aroused with longer times is much lower. The same thing is true also of the duration of 75 $\sigma$  except in one instance, namely C 100, where the number of full movements aroused compared favorably with those reported when the duration was 150 $\sigma$ . (c) The stronger of our two intensities, the 'subcutaneous' stimulus, yields a slightly larger percentage of full movements with its best interval, 75 $\sigma$ , than does the cutaneous stimulus at its best interval, 100 $\sigma$ .

(4) Whereas Dimmick was able to secure the perception of visual movement, under optimal conditions, in practically 100% of his trials, we found that the perception of cutaneous movement appeared, under the same conditions, in only 57 to 67% of our observations. Since, so far as we can discover, there is nothing in the arrangement of the two experiments that accounts for this difference of result, we are forced to the conclusion that the cutaneous perception is less fundamental and compulsory (in Dimmick's sense) than the corresponding visual perception.

(5) Support for this view is found in a comparison of the existential correlates of the two perceptions. Dimmick's grey flash is an integration of quality with time; space is not involved. The existential correlate of the cutaneous perception appears, on the other hand, to be an integration of quality, time and cutaneous extent. While Dimmick's O's reported a curtain or film of shimmering, liquid, live grey, our O's (in part the same as Dimmick's) report a pressure diffusing, growing, extending, in time. The integration appears to be of a higher order, and therefore less stable and inevitable.

(6) We regard it as entirely premature to theorise from the results of Wertheimer and Dimmick in vision and from our own results in touch to 'the' perception of visual or cutaneous movement. It may very well be that the singular number must be replaced in this and similar connections by the plural. 'Movement' is, after all, a gross meaning that may be carried by several existential correlates; or, to put the same thing in other words, an organism whose outfit of sense-organs is as complex as our own may be adequate to the 'perception of movement' by way of several different primary integrations.

# SOME QUALITATIVE ASPECTS OF BITONAL COMPLEXES<sup>1</sup>

By CARROLL C. PRATT

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The present investigation neither pretends to a resolution of the problem of fusion in its systematic context nor to a new determination of the order of degrees of fusion. It was initiated under the conviction that there were within reach a point of view and a method, which would single out for separate examination certain phenomenological characteristics which enter into the total impression set up by two tones when presented simultaneously to the organ of hearing. The literature on tonal fusions, although rich in experimental studies undertaken from various angles of approach, is nevertheless far from being in a settled state. The suspicion is hard to escape that much confusion has resulted from a failure to take into consideration the fact that any judgment upon the nature of fusion is inevitably ambiguous unless directed upon one univocal characteristic of the tonal impression.

A review of the literature would be unnecessary iteration in the present article.<sup>2</sup> Many of the controversies to which Stumpf's classic volumes gave rise are due to Stumpf's ambiguity of definition and procedure, or at best, to a misunderstanding on the part of others of his exact concept of fusion. It is not surprising that misunderstandings should arise. As a rule Stumpf displays a predilection for calling fusion "dasjenige Verhältnis zweier Inhalte, speciell Empfangungsinhalte, wonach sie nicht eine blosse Summe sondern ein Ganzes bilden."<sup>3</sup> Nevertheless by way of amplification, Stumpf refers to fusion as "dasjenige Verhältnis zweier Empfindungen, in folge dessen" the total impression of the more perfect fusions "immer mehr dem Einer Empfindung nähert und immer schwerer analysirt wird."<sup>4</sup> Of

<sup>1</sup> From the Psychological Laboratory of Clark University.

<sup>2</sup> For a recent review of the literature, see W. Kemp, *Methodisches und Experimentelles zur Lehre von der Tonverschmelzung*, *Arch. f. d. ges. Psychol.*, 1913, 29, 139-181.

<sup>3</sup> C. Stumpf, *Tonpsychologie*, 1890, ii, 128.

<sup>4</sup> *Loc. cit.*

course such aspects of fusion are secondary, but Stumpf uses them in his experiments as criteria for fusion. Again Stumpf calls fusion "eine unveränderliche Eigentümlichkeit des Empfindungsmateriales, welche immer noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden, und welche gerade nachdem die Analyse vollzogen und die Töne deutlich als zwei erkannt sind, ebenfalls erst in sich selbst bemerkt werden kann."<sup>5</sup> "Wie sich dies ausnimmt, muss man eben hören und kann es Niemand klarmachen, der nicht hören oder Gehörserscheinungen nicht beobachten kann."<sup>6</sup> Stumpf throws in another confusing implication when he remarks that "der Zusammenklang zweier Töne nähert sich bald mehr, bald weniger dem Eindruck eines Tones, und es zeigt sich, dass dies um so mehr der Fall ist, je konsonanter das Intervall ist."<sup>7</sup> Wundt<sup>8</sup> has brought the reproach against Stumpf of creating a purely logical concept of fusion, in spite of Stumpf's manifest efforts to base his inductions on phenomenological experience. Others,<sup>9</sup> accepting the Stumpfian doctrine on its theoretical side, or, on the contrary, dispensing with it, or at times modifying it, have centered their interest in the search by repeated experiment and observation for a more stable order of degrees of fusion. Still others<sup>10</sup> doubt the justification for introducing a unique notion like fusion into the psychological system and attempt to account for the phenomenon in terms of analysability, unitariness, harmony or what not, or else to work out an entirely new definition of fusion.

Kemp's experiments are an attempt to separate out the factors which may enter into the phenomenon of consonance. The results indicate the operation of the following four *Merkmale*: fusion in the sense of that "unveränderliche Eigentümlichkeit welche immer noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden;" pleasantness and unpleasantness; *sinnlicher Wohlklang*; *sinnliches Zusammenpassen*, a sort of relationship by virtue of which the components of an interval seem to belong or go together; and *harmonisches Zusammenpassen*, the need of resolution (*Auflösungsbedürfniss*).<sup>11</sup> Unfortunately, however, Kemp gives no clue as to the structural na-

<sup>5</sup> *Ibid.*, 127f.

<sup>6</sup> *Konsonanz und Dissonanz*, 1898 44.

<sup>7</sup> *Ibid.*, 35.

<sup>8</sup> *Physiologische Psychologie*, 1910, 6 Aufl., ii, 124ff.

<sup>9</sup> E.g., O. Külpe, *Grundriss der Psychologie*, 1893, 289ff.; A. Faist, *Versuche über Tonverschmelzung*, *Zeits. f. Psychol.*, 1897, 15, 102ff.; Th. Lipps, *Leitfaden der Psychologie*, 1909, 3 Aufl., 98ff; *Tonverwandtschaft und Tonverschmelzung*, *Zeits. f. Psychol.*, 1898, 19, 1ff; W. Kemp, *op. cit.*, 153; T. H. Pear, The experimental examination of some differences between the major and minor chords, *Brit. J. Psychol.*, 1911, 4, 56ff.

<sup>10</sup> Wundt, *Phys. Psychol.*, 1910, ii, 121; F. Krueger, *Beobachtungen an Zweiklängen*, *Philos. Stud.*, 1900, 16, 307ff; *Die Theorie der Konsonanz*, *Psychol. Stud.*, 1906, 1, 305ff; 1907, 2, 205ff; 1909, 4, 201ff; 1910, 5, 294ff; P. Natorp, *Göttinger, Gel. Anz.*, 1886, 145ff; 1891, 789ff; H. Cornelius, *Ueber Verschmelzung und Analyse*, *Vierteljahrsschr. f. wiss. Philos.*, 1892, 16, 404ff; E. Buch, *Ueber die Verschmelzung von Empfindungen, besonders bei Klangeindrücken*, *Philos. Stud.*, 1899, 15, 1ff, 183ff.

<sup>11</sup> Kemp, *op. cit.*, 181-203.

ture of these *Merkmale*: an omission regrettably common in work on the psychology of fusion.

The present experimental investigation was begun in the fall of 1919 and continued until the spring of 1921 in the Psychological Laboratory of Clark University.

#### PRELIMINARY SERIES

The *O*'s who took part in the experiment were G, Dr. E. G. Boring, who worked with knowledge of the problem; F, Dr. S. W. Fernberger; L, Dr. L. D. Boring; Y, Dr. M. Yokoyama; and B, Miss M. Bates. All were highly trained in observation. B was the most "musical" *O*, but found no difficulty in abstracting from musical knowledge. L had absolute recognition of single pitches but had no musical recognition of intervals. Y had but slight "ear" for occidental music and could not recognize intervals musically at all. G and F had no musical recognition of intervals.

Three Stern variators (210-400, 310-600, and 410-800 d.v.) were used as stimuli throughout the experiments. They were placed side by side on a wooden support 80 cm. from the floor on one side of the experimenting room. *O* sat with his back toward the source of sound at a distance of 1.5 m.

Preliminary experiments were conducted to determine roughly the rank order of degrees of fusion for the five *O*'s on the basis of rapidity of analysis, a method suggested by Külpe.<sup>12</sup> A Bergström chronoscope, placed alongside the variators and connected with the reaction key on the arm of *O*'s chair, was used to measure reaction times. The octave, fifth, fourth, sixths, thirds, tritone, sevenths, and seconds were used. The order of presentation was varied from time to time, and occasional *Vexirversuche* of single tones were thrown in. Fifty reactions upon each interval were taken.

The following typewritten instructions were read by *O* at the beginning of each session: "Following the 'ready' signal, you will be presented with (a) single notes, and (b) two-note complexes. The duration of the stimulus will be 0.5 sec. You are requested in each case to react upon the key as soon as you have decided whether the sound which you hear is that of one note or two notes, stating afterwards whether the judgment was 'one,' 'two,' or 'doubtful.' In case of the latter, indicate, if possible, the direction in which the judgment inclined."

Proceeding on the uncritical assumption that degrees of fusion are inversely proportional to rapidity of analysis, a scale of degrees of fusion was calculated from the reaction times of each *O*. The rank orders thus obtained were utilized in the next step of the preliminary experiments.

The purpose in this second part of the preliminary experiments was to present tonal complexes to *O* which could not readily be analyzed into separate components and to secure from *O* qualitative descriptions of the impressions thus set up. Although Stumpf<sup>13</sup> insists that the phenomenon of fusion still persists after analysis is complete, it was deemed worth while to forestall analysis at least in part by combining the intervals which had yielded less quickly to analysis in the preceding experiments into triads that could be but very imperfectly separated. (*E.g.*, the first six intervals which resulted from the foregoing experiments for L were in order of difficulty of analysis: octave,

<sup>12</sup> *Grundriss der Psychologie*, 1893, 239.

<sup>13</sup> *Tonpsychologie*, ii, 127f.



fifth, fourth, major third, minor sixth, major sixth. These intervals were combined into the following triads: d-g<sup>b</sup>-a, major third and fifth; c-g-c, fifth and octave; d-g-d, fourth and octave; d<sup>b</sup>-f-b, major third and major sixth; a-f-a, minor sixth and octave; b-a<sup>b</sup>-b, major sixth and octave; d-g-b, fourth and major sixth; and c-e-c, major third and octave. Similarly for the other O's.)

In these experiments the stimuli were allowed to sound for one sec. *Vexirversuche* of two-note complexes were occasionally interspersed. The instructions were similar to the previous experiments, except that the phrases "two-note" and "three-note complexes" were substituted for "single notes" and "two-note complexes." It was not long before the judgments and the length of reaction times furnished *E* with sure indication that certain of the triads were submitting very imperfectly to analysis. Judgments of "two" for three-note complexes were very frequent; or the time of reaction was often so great that *E* lost count of the number of swings of the pendulum across the 2000 $\sigma$  scale. *E* secured introspective descriptions of the impressions aroused by these more imperfectly analyzable triads. The qualitative data thus obtained put *E* in the possession of valuable descriptive material for experiments to be mentioned presently.

It now appeared desirable to adopt a direct method of observation.<sup>14</sup> The method of paired comparisons provided a suitable form of procedure. Every one of the twelve intervals was paired with the eleven other intervals six times, making thus  $(12 \times 11) / 2$  comparisons  $\times$  6 repetitions = 396 judgments in all. There were 11 comparisons  $\times$  6 repetitions = 66 total possible preferences for any one interval. The intervals were arranged in haphazard order, with the precaution that no interval should be presented twice in succession. The components for the intervals were all included within the range of a major 9th, from b<sup>b</sup> of the small octave (234.66 d.v.) to c<sup>2</sup> of the twice-accented octave (528 d.v.). With an eye to the time-error, the order of presentation of the intervals within a pair was reversed in alternate series. It was possible to secure about 100 judgments in each experimental session.

The following instructions, derived from the Stumpfian *Folgeerscheinung* of *Einheitlichkeit*, were read by *O* at the beginning of every session: "Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of unitariness, regardless of other possible characteristics of the impressions. Report 'first' or 'second' according as the first or second impression is more unitary. If there is no difference between them, report 'same.'"

These instructions proved to be highly equivocal for all O's, for they soon discovered that the meaning of unitariness might be carried by quite different sensory data present in the total tonal impression. The judgments were made only with extraordinary difficulty and reported with still greater reluctance, for *O* was peculiarly loth to trust his judgment when faced with several processes all of which were possessed of potential contexts for unitariness in nearly equal degrees of mediacy.

<sup>14</sup> On direct vs. indirect methods in work on tonal fusions, see Kemp, *op. cit.*, 163-179.

The situation thus created is interesting in the light of recent attention to the "stimulus-error" in psychophysics.<sup>15</sup> Ordinarily a careful control of attitude is necessary to secure univocal determination in psychological experiment. The experiments of George<sup>16</sup> indicate the ease with which equivocal criteria may arise with inconstancy of attitude, and the recent work of Friedländer<sup>17</sup> and Fernberger<sup>18</sup> with lifted weights makes clear the effect of this inconstancy upon the objective results which psychometric functions represent. The manner in which the limens for dual impression upon the skin may vary under attitudinal shifts has become well established in cutaneous psychophysics.<sup>19</sup> This development simply serves to give impetus to the current urge in experimental work for a sharper delineation of observational attitudes, to the end that a more univocal correlation may be arrived at between the terms in the total chain of experience. This chain, following Urban's<sup>20</sup> schema, may be represented by  $\alpha, \beta, \gamma$ , . . . for physical conditions (stimulus-situation),  $a, b, c$ , . . . for physiological processes of nerve excitation, and  $A, B, C$ , . . . for contents of consciousness. Since similar terms in these three systems, as  $\beta, b, B$ , are not in experimental practice univocally correlated, it becomes necessary to discover what factors in the  $A, B, C$ , . . . system are correlated, e. g., with  $\beta$  in the stimulus-situation; and it may turn out that the conscious correlates of  $\beta$  are  $A, C, M$ , and  $P$ , depending on the nature of the *Aufgabe* which is operative in the stimulus-situation. If the *Aufgabe* is not set carefully by instructional determinants,  $O$  may judge now upon one, now upon another, of the  $A, C, M, P$ , factors, so that it becomes scientifically absurd to speak of a correlation. Hence "the total *Aufgabe* under which the subject judges must be made definite in instructions, and must be more fully determined by means of repeated characterizations by the subject of his attitude and procedure. This latter check is important since much of the subject's instruction is apt to be a self-instruction."<sup>21</sup>

In the series of experiments under consideration, it became apparent that the  $O$ 's were laboring under an equivocal instructional determinant. Let it be supposed, for instance, that the tonal intervals in the stimulus-situation had among other conscious correlates the factors  $B, D, L$ , and  $Q$ , all of which were potentially associated with the contexts  $B', D', L'$ , and  $Q'$ , carrying the meaning for unitariness.

<sup>15</sup> See especially E. G. Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 1920, 27, 446ff., 449; The stimulus-error, *Amer. J. Psychol.*, 1921, 32, 449ff.

<sup>16</sup> S. S. George, Attitude in relation to the psychophysical judgment, *Amer. J. Psychol.*, 1917, 28, 1ff.

<sup>17</sup> H. Friedländer, Die Wahrnehmung der Schwere, *Zeits. f. Psychol.*, 1920, 83, 129ff.

<sup>18</sup> S. W. Fernberger, An experimental study of the "stimulus-error," *J. Exp. Psychol.*, 1921, 3, 63ff.

<sup>19</sup> E. B. Titchener, Ethnological tests of sensation and perception, *Proc. Amer. Philos. Soc.*, 1916, 55, 206-215; E. deLaski, Perceptive forms below the level of the two-point limen, *Amer. J. Psychol.*, 1916, 27, 569-571; C. L. Friedline, Discrimination of two cutaneous patterns below the two-point limen, *ibid.*, 1918, 29, 400-419; and especially Boring, The stimulus-error, *Amer. J. Psychol.*, 1921, 32, 449ff.

<sup>20</sup> F. M. Urban, Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.*, 1913, 30, 116-118.

<sup>21</sup> Boring, *Psychol. Rev.*, 27, 449.

Under the *Einstellung* laid down by the instructional determinant for unitariness, *O* would have a wide and unchecked range for equivocal judgments which would issue in rank order of degrees of unitariness of which the scientific significance would be small.

CHART 1

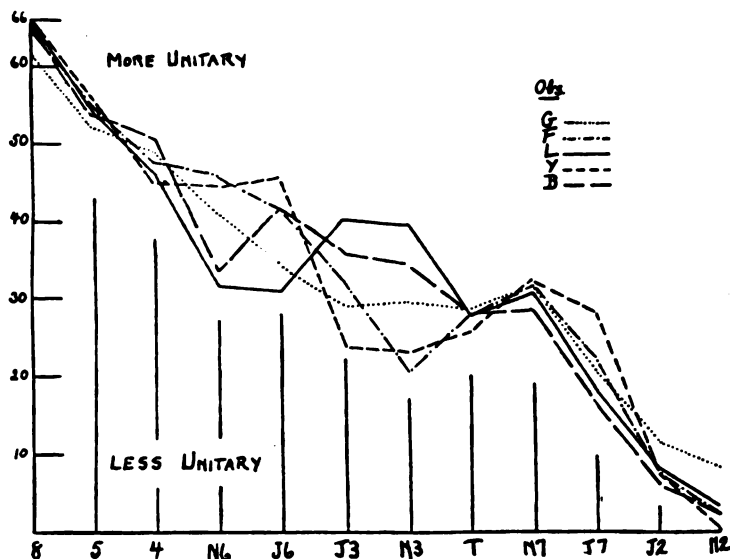


CHART 1. Rank-order of tonal intervals under instructions for 'unitariness' (see text). Method of paired comparisons. Ordinates represent number of discriminations for every interval for five *O*'s. Abbreviations: 8 = octave, 5 = fifth, 4 = fourth, N6 = minor sixth, J6 = major sixth, J3 = major third, N3 = minor third, T = tritone, N7 = minor seventh, J7 = major seventh, J2 = major second, N2 = minor second.

In Chart 1 are shown the curves derived from judgments of unitariness. In all the charts the intervals on the abscissa have been arranged in the same order for ease of intercomparison. The numbers on the ordinate represent the frequency of preferences. The curves of Chart 1 make clear the rather significant fact that *O*'s can take up a stimulus-attitude toward mental experience and come out with a surprising degree of uniformity. The net returns to psychology of such agreement of results are small, however, inasmuch as no light is thrown upon the mental processes involved in the judgments.

At the end of this series of experiments, the *O*'s were asked for introspective characterizations in which should be noted the criteria which had entered into or cut across the judgments of unitariness. These reports, together with the qualitative data secured from the earlier preliminaries, furnished the material out of which grew the experiments now to be considered.

### PRINCIPAL SERIES

The method and procedure in the following series were the same as in the foregoing experiment. The only difference lay in the instructions given to *O*. From the introspective data at hand were selected those factors in the *A, B, C, . . .* system of tonal mental processes which approximated the phenomenological level of experience. It was hoped by means of instructions and self-checking on the part of *O* to limit the judgments to a single factor, and thus come out with rank orders of which the significance would be more certain. Certain of the factors employed as criteria of judgment were common to the introspective reports of all the *O*'s; others, especially those of a contextual nature, followed the idiosyncrasies of individual differences and could be used as instructional determinants only for particular *O*'s. Before beginning work on a new criterion, one observational session was devoted to practice judgments, in order that *O* might familiarize himself with the new factor and become sufficiently *eingestellt* to take up a similar attitude in subsequent sessions. Whenever, during the series, *O* noted a lapse in attitude or found it impossible to make a judgment in accordance with instructions, he was expected to report the fact to *E*. At the end of every series *O* was requested to give a descriptive characterization of his attitude and basis of judgment.

### *Auditory Processes*

*Smoothness-roughness.*—The first criterion to be used had as its sensory basis the 'smoothness' and 'roughness' of the auditory impressions. This factor had been frequently reported in all the introspections, and was easily taken up and followed through by all the *O*'s. The instructions for this series read as follows:

"Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of 'smoothness' and 'roughness,' regardless of other possible characteristics of the impressions. Report 'first' or 'second' according as the first or second impression is 'smoother' or less 'rough.' If there is no difference between them, report 'same.' Whenever you fail to live up to instructions or find it impossible to make a judgment on the basis of the above criterion, report 'failure.'"

The purpose of such an instruction was to get *O* univocally determined in the direction of auditory 'smoothness-roughness' and away from other possible factors in the tonal impression.

The descriptive characterizations point to 'roughness' as the positive category in this series, and to 'smoothness' as the absence of 'roughness.' 'Roughness' is a temporal-intensive pattern in which intensity varies in time, whereas complexes in which intensity seems relatively constant are 'smooth.' Comparison-judgments between complexes in which the variation of intensity is rapid and those in which intensity variations are slow (minor thirds and major seconds *vs.* minor seconds) embarrassed some of the *O*'s. This difficulty they usually resolved by judging as 'smoother' those complexes in which the intensity variations are more rapid.

CHART 2

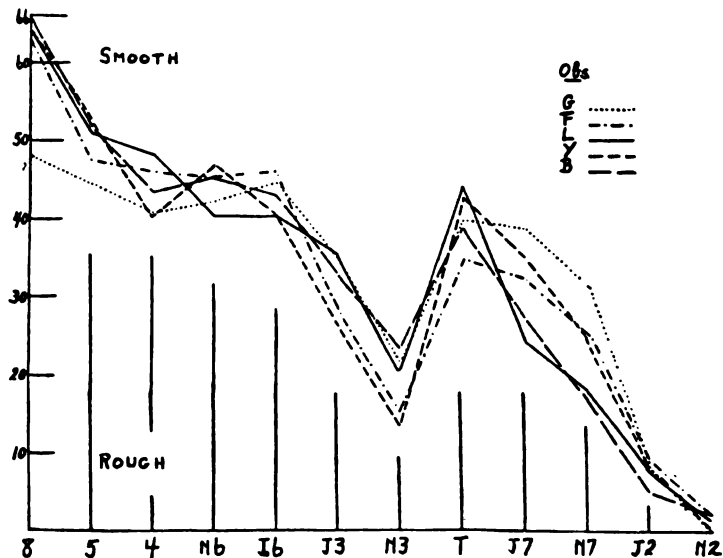


CHART 2. Rank-order of tonal intervals under instructions for 'smoothness-roughness' (see text). Curves for five *O*'s.

Chart 2 indicates the rather striking uniformity of rank orders obtained under this attitude. It will be noted that 'smoothness' and 'roughness' vary with the physical distance of the components of the interval: the octave, fifth, fourth, sixths, tritone, minor seventh, and major third all come higher



on the scale than the minor third and the seconds. The relatively low position of the major seventh must be due to the presence of overtones which cut across the factor of distance.

*Simplicity and Complexity.*—The next two criteria of judgment to be adopted took their root in the general phenomenon of analysability. The possibility of taking up two attitudes in this respect made it advisable to follow through separate series. The instructions in the first series, as, in point of fact, for all subsequent series, were worded like the instructions for 'smoothness-roughness,' except in those phrases intended to determine *O* in a new direction:

"Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of 'simplicity.' By 'simplicity' is meant the degree of approximation to singleness of auditory core, i. e., unanalysability of the impressions. Report 'first' or 'second' . . . " etc.

The instructions for the second series read:

". . . In each case you are to compare the two impressions with respect only to degrees of 'complexity,' i. e., you are to state in which of the two impressions the components separate most during presentation. . . ." <sup>22</sup>

The introspective data for the 'simplicity' series indicate that the criterion here involves difficulty of analysis or independent variability of clearness of components in time, which occurs under a fairly active attitude (=attentional attempt at analysis). This is to say that those complexes in which it takes longer for the total impression to split up into its separate components, or in which one component dominates the other in clearness, are judged as more 'simple' inasmuch as they approximate more nearly to singleness of auditory core.

In the 'complexity' series, on the other hand, the judgments are made under a more passive attitude. *O* reported merely upon the relative "analysedness" of the impressions as given during presentation of the stimulus, and not upon 'analysability.'

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<sup>22</sup> During the practice period for this series it was discovered that the *O*'s were often uncertain in their judgments because of the change which the impressionable contents for judging underwent during the formulation of the judgments in the course of the immediate memory after-images. Hence they were instructed to base their judgments upon the contents as they appeared *during presentation*. To facilitate this mode of judgment, the exposure time of each interval was reduced from 2. to 0.5 sec. (measured by a silent metronome).

In Chart 3 are shown the curves for the 'simplicity' series. The 'simplicity' curves for G and B turned out to be nearly identical with their 'complexity' curves, and have therefore been placed as G(s) and B(s) on Chart 4 ('complexity') instead of on Chart 3 (the other 'simplicity' curves). The three curves of Chart 3 follow fairly closely the Stumpfian order of degrees of fusion, except for the rather high positions of the tritone and minor seventh, a possible explanation of which will be mentioned presently. The 'complexity' curves deviate from the 'simplicity' curves in a uniform and significant manner. Those intervals of which the components lie far apart on the tonal scale (sixths and sevenths) occupy the lower positions on the chart, whereas those intervals of which the components are proximate (thirds and seconds) occupy uniformly higher positions. The high positions of the octave, fifth, and fourth, are undoubtedly referable to the factor of 'fusion'<sup>23</sup> which cuts across the factor of sense-distance. The opposed positions of the seconds on the two charts demand a word of explanation, an explanation forthcoming in the introspections, which show quite definitely that two different attitudes are operative in the case of the seconds: a 'meaning' attitude and a descriptive attitude. The O's whose curves for the 'simplicity' series are represented in Chart 3 judged certain of the intervals 'less simple' on the basis of contents which carried the meaning of non-simplicity rather than on the basis of relative degrees of simplicity of the auditory experience *per se*. The following quotations from their introspective characterizations illustrate the point:

Ob. F. "In some cases one of the complexes comes as very rough. It has the meaning of being readily analyzable at once. How this meaning is present structurally I cannot say, although the basis of it seems to be roughness. There is a verbal characterization of it as 'less simple,' but there does not seem to be any auditory attentional analysis."

Ob. L. "I tend naturally to judge as not simple a complex which produces roughness, or one where the 'noise' stands out prominently."

Ob. Y. "In such a case [comparison of a rough complex with a smooth one, the components of which stand out clearly] the rough one is usually cognized as a complex of two tones, more immediately than the other. Yet, since it was very difficult to split up the rough interval into two components actually, *i. e.*, psychologically, I judge it as simpler."

<sup>23</sup> The word "fusion," when used in connection with the curves under consideration, bears no theoretical implications: it is merely a convenient term to apply to those intervals whose components either will separate out only with difficulty (=unanalysability) or do not fall apart during presentation (=unanalsedness).

Y's report shows that he was aware of his two attitudes, but his curve points to only a partial approximation to the psychological descriptive attitude: more often he judged the rough intervals on the level of cognizance and meaning. In the complexity series, however, he was able to abide quite rigidly by the descriptive attitude, as his curve indicates. L, too, was able to keep to the descriptive attitude fairly consistently in the 'complexity' series, although not to the extent

CHART 3

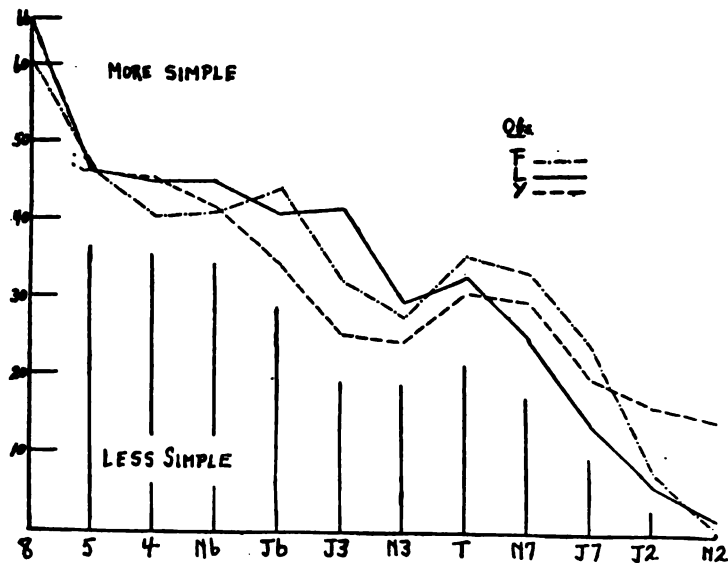


CHART 3. Rank-order of tonal intervals under instructions for 'simplicity' (see text). Curves for three O's. 'Simplicity' curves for two O's are shown in Chart 4.

of the other O's; at least the low position of her curve at the seconds would seem to warrant such a statement. It is matter for regret that circumstances made it impossible for F to complete the 'complexity' series.

Regarding the high positions of the tritone and minor seventh in the 'simplicity' series, adverted to above, it may be that this very factor of attitude is operative. In the 'smooth-rough' series the tritone and minor seventh occupy relatively high places, and since 'roughness' was part-mediator for the meaning of 'less simple,' it is reasonable to sup-

pose that 'smoothness' may have entered in at times to support the meaning of simplicity, and thus give to the tritone and minor seventh values higher than would otherwise have been the case. It is not clear, however, why for two of the O's the 'simplicity' series gave curves like the 'complexity' curves. A valid explanation can be sought only in repeated experiments under more stringent control of instructional determinants.

CHART 4

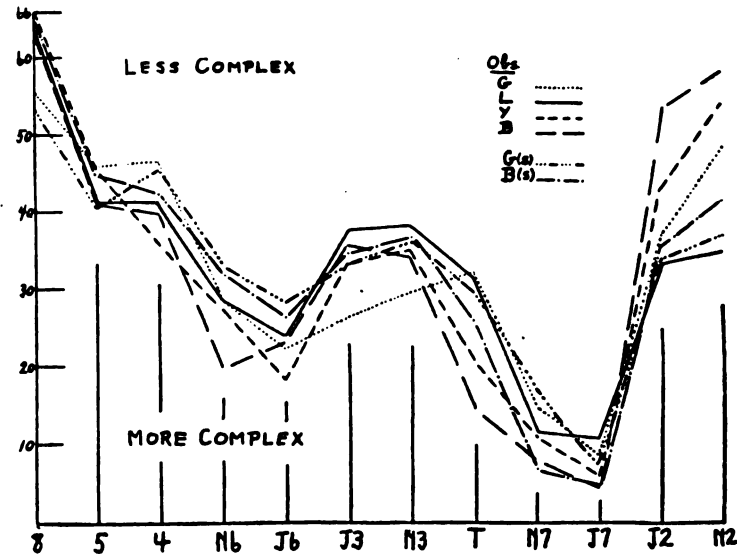


CHART 4. Rank-order of tonal intervals under instructions for 'complexity' (see text). The curves for G(s) and B(s) are 'simplicity' curves (see Chart 3). The four 'complexity' curves are inverted.

*Pleasantness and Unpleasantness.*—Although in the preliminary experiments, the O's had stated that pleasantness and unpleasantness entered but seldom as disturbing criteria of judgment, it was deemed advisable to run through a series of P and U in order to discover just how judgments would come out under an affective determination. The instructions were similar to previous ones with the exception that O was requested to judge in terms of P and U only.

Under the conditions of the present experiment, P and U

turn out to be meanings for all O's,<sup>24</sup> or at least experiences not *sui generis*. For F and Y, the introspective reports state the potency of 'smoothness' and 'roughness' as mediating factors in the affective judgment, a fact borne out by the resemblance of the curves of F and Y (Chart 5) to the 'smooth-rough' curves. The agreement of these two curves and the agreement of the other three curves, and the marked difference between the two types, are worthy of especial note.

CHART 5

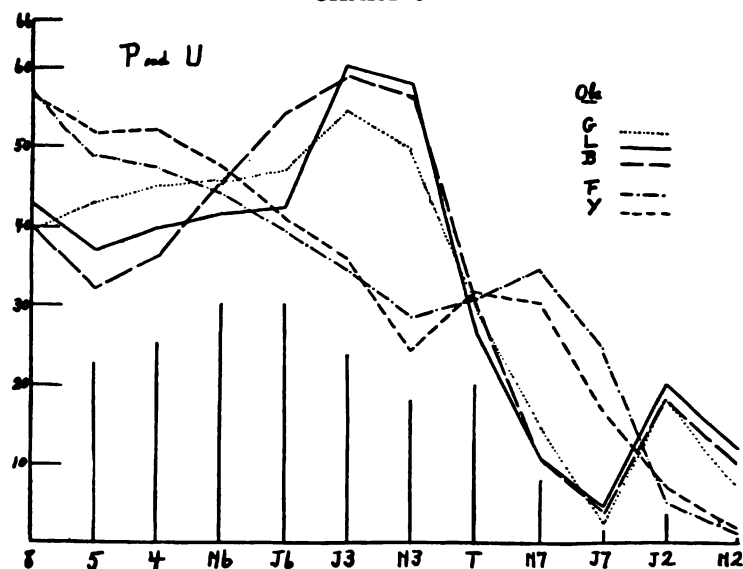


CHART 5. Rank-order of tonal intervals under instructions for P and U. Curves for five O's.

For G, L, and B, whose curves represent the second type, the P and U judgments are explicitly reported as being on the level of meaning. Just what the sensory mediators for the judgments represented by these curves may be can not be satisfactorily gleaned from the introspective reports, although hints are not wanting to point to non-auditory concomitant processes of the auditory experience as playing a part. (See below: the "horrisonorous" curve for G, the "rotundity"

<sup>24</sup> For a recent exposition of the resemblance between affection and meaning, see M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, *Amer. J. Psychol.*, 1921, 32, 357ff.



and "hard-soft" curves for L, and the "kinaesthesia" curve for B.) Perhaps the most interesting feature of these curves is their consistency with musical usage: the sixth and thirds are very pleasant; the minor seventh and seconds are unpleasant and the major seventh very unpleasant; the octave, fifth and fourth are rather "indifferent."

So far the judgments under varying attitudes have come from instructions the purport of which was to determine *O*

CHART 6

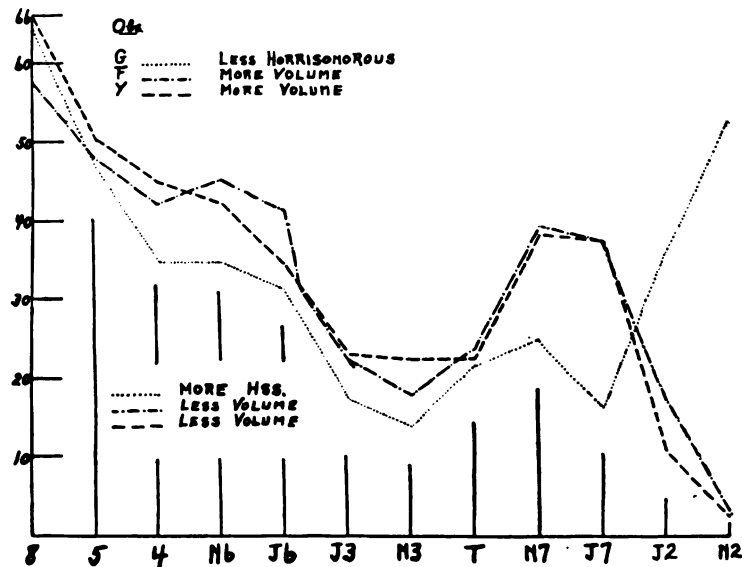


CHART 6. Rank-order of tonal intervals under criteria into which entered integrations of auditory and non-auditory processes (see text). Curves for three *O*'s. The curve for *G* is inverted.

in the direction of purely auditory aspects of the total contents set up by the stimulus-situation. In the qualitative analyses of the preliminary experiments, however, there were numerous references to non-auditory concomitant processes, or to integrations of auditory and non-auditory processes, which presented themselves as important parts of the whole impression or as strong candidates for judgment in the case of unitariness. For the sake of greater completeness, therefore, although an exhaustive study of all possible factors in the *A*, *B*, *C*

. . . system was of course out of the question in the time at our disposal, it was decided to follow through certain of the more insistent contextual processes.

*Integration of Auditory and Non-auditory Processes*

The curves of Chart 6 represent rank orders from judgments based on an intimate integration of auditory and non-auditory processes. The criterion of this sort followed through by F was integration of the voluminous aspect of the auditory impression and a concomitant chest-kinaesthesia of varying degrees of expansiveness; a similar criterion by Y, an integration of the same voluminous factor and a concomitant visual process between the two extremes of which a gap of varying width could be observed. In addition to the close uniformity of the two curves there should be noted also the factor of distance of components on the tonal scale, which is again operative: the low position of the thirds and seconds in which the paired components are close together, and the high position of the sevenths and sixths in which the paired components are farther removed from each other. Otherwise the form of the curves is similar to that for the 'simplicity' series. The criterion adopted by G in this group was an integration of auditory and kinaesthetic processes to which, for the sake of convenient reference, he originally gave the term "horrisonorous." By this term he meant the vibrant, rather unpleasant, factor in the auditory experience (*horridus*) together with the auditory fulness and richness of the impression. Although on closer observation G became convinced that the pattern of "horrisonorousness" consisted of an intimate integration of ear-kinaesthesia and auditory quality which could only be separated under careful attentive analysis, he still retained the term. The form of G's curve is similar to those for F and Y, and what has been said concerning these latter is equally true of the former. The one exception, in the case of the seconds, is explained in G's introspections by the fact that with the coming in of marked roughness the "horrisonorous" factor is 'swamped.' (The curve has been inverted on the chart.) Were it not for this rapid rise of the curve at the seconds, the resemblance to the other two curves would be still greater, inasmuch as a lowering of the curve at one point would result in an elevation at another, the essential event in this case for bringing about a greater agreement.

*Concomitant Non-auditory Processes*

Chart 7 gives the curves for judgments based on purely non-auditory concomitant processes. L, who is of a marked visual type, was able to make judgments on various aspects of what she called a 'pseudo-visual' image, *i. e.*, kinaesthesia of eye-movement which served as an effective surrogate for the actual visual experience. By 'rotundity' she meant the fulness and size of the visual image; by 'hard-soft,' the texture

CHART 7

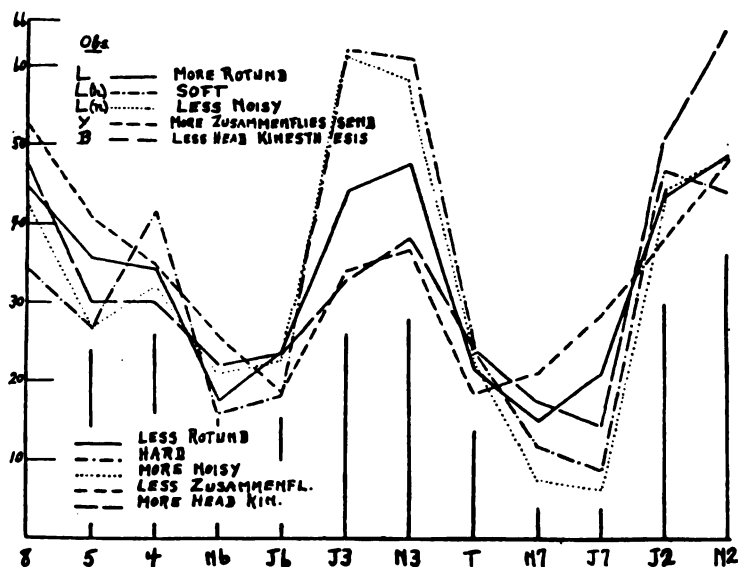


CHART 7. Rank-order of tonal intervals under criteria into which entered concomitant visual and kinaesthetic processes (see text). Curves for three O's. The curves for L(h), L(n), and B are inverted.

of the image; and by 'noise component,' the jagged or even contour of the image. Y followed through a criterion which he called visual *Zusammenfliessen*, by which he meant the pulling together or apart of concomitant visual images in the field of vision. By 'head-kinaesthesia' B referred to varying degrees of extension of concomitant kinaesthesia across the posterior portion of the head. It can be seen at a glance that there is less agreement among the curves of Chart 7; nevertheless a marked directional uniformity is apparent at the

TABLE I

FREQUENCY OF JUDGMENTS UPON TONAL INTERVALS UNDER VARIOUS CRITERIA. METHOD OF PAIRED COMPARISONS. COLUMN AT LEFT REFERS TO PARTICULAR CRITERION OF JUDGMENT. SEE CHARTS AND TEXT. ABBREVIATIONS: 8 = OCTAVE, 5 = FIFTH, 4 = FOURTH, J = MAJOR, N = MINOR, T = TRITONE

Criterion	Obs.	8	5	4	N6	J6	J3	N3	T	N7	J7	J2	N2
Chart 1 Unitariness	G	61½	52	49	41	34	29	29½	28½	31½	20½	11½	8
	F	64½	55	47½	45½	41½	32	20½	27½	31½	22	7½	1
	L	66	54½	46	31½	31	40	39½	27½	30½	18½	8	3
	Y	66	55½	45	44½	45½	23½	23	25½	32	28	7½	0
Chart 2 Smooth-Rough	B	65½	54	50½	33½	41½	35½	34½	28	28½	16½	6	2
	G	48	44½	41	42½	44½	35	21½	40	39	31½	8½	0
	F	63	47½	46	45½	46	28½	15½	35	32½	25½	9	2
	L	64	51	48½	40½	40½	35	21	44½	24½	18	7½	1
Chart 3 Simplicity	Y	64	52½	40½	47	40½	26½	13½	43	35	25	8½	0
	B	65½	51½	43½	45½	43	33	23½	39	27	17½	5	2
	F	60½	46½	40½	41	44	32½	27½	35½	33½	24½	8½	1½
	L	66	46½	45	45	41	41½	29½	33	25	14	7	2½
Chart 4 Complexity	Y	66	46	45½	41½	34½	25½	24½	31	30	20	16½	15
	G (s)	53½	40½	45½	33½	28½	33½	36	29½	17	7½	34	37
	B (s)	65	45	42	32	26½	34½	36½	25½	7	5	35½	41½
	G	55½	46	46½	28½	22½	26½	29½	32	15	9	37	48
Chart 4 Complexity	L	64	41	41	28½	24	37½	38	31½	11½	11	33½	34½
	Y	66	45½	35½	27½	18½	33½	35	20½	11	6	43	54
	B	63½	41	40	20	23	35½	34½	14½	8	4½	53½	58





position of certain of the intervals. As in Chart 6 the 'distance' factor is operative here, although in the opposite direction: the thirds and seconds (intervals whose components are proximate) rank relatively high, and the sevenths and sixths (distant components) low. Unusually low positions of the octave, fifth, and fourth are undoubtedly due to the violence with which the 'distance' factor intrudes upon the 'fusion' factor: note the extremes attained by the thirds and sevenths.

#### CONCLUSIONS

The numerical data from which the charts have been drawn are summarized in Table 1. The fractional numbers there derive from the usual procedure in the method of paired comparisons of giving equal value (*i. e.*,  $\frac{1}{2}$ ) to each member of a pair in an equality judgment.

The results indicate clearly the complexity of the tonal impression that is dependent upon the presentation of bitonal stimuli. Various factors which make for this complexity may be isolated by means of carefully controlled instructional determinants and examined apart. Such isolation and examination as the present study has undertaken bring to view certain of the phenomenological characteristics which constitute the foundation of bitonal impressions.

*Smoothness-roughness.*—The continuum to which the expression 'smoothness-roughness' applies is best described in terms of 'roughness.' By 'roughness' the *O*'s meant a temporal-intensive pattern in which various rates of intensive oscillations of the auditory experience are observable. 'Roughness' is inversely proportional to the rapidity of intensive oscillations. 'Smoothness' is the absence, or relative absence, of intensive oscillations. Absolute 'smoothness' is a continuation in time of the auditory experience without observable variations in intensity.

*Simplicity.*—By 'simplicity' the *O*'s intended the phenomenon of unanalysability. The introspective reports show that by unanalysability they meant the covariation in sensory clearness of the auditory components under an active attitude (=attentional analysis) by virtue of which isolation of one component becomes difficult, that is to say, occurs only incompletely or only after a considerable interval, or else, on occurring, persists only for a short time. The tonal complexes are judged increasingly less 'simple' as the variation in clearness of the components becomes more independent.

*Complexity.*—For judgments of 'complexity' the *O*'s assumed a more passive attitude, *i. e.*, an attitude for judging

upon the 'complexity' of the interval as it appeared *during presentation*, rather than an active attempt at attentional analysis. From the introspective characterizations it becomes apparent that the *O*'s understood by 'complexity' the degree of disparity (nearness or remoteness on a scale of 'sense-distance') of the auditory pitch qualities as they appear in the tonal impression. Those intervals in which the components are readily perceived as disparate with respect to pitch quality are judged as 'complex,' whereas those intervals in which the pitch qualities are less readily perceived as separated or dual are judged as less 'complex.'

From a consideration of Chart 4 it becomes apparent that 'complexity' varies with physical distance: those complexes in which the components lie further apart on the tonal scale are judged more 'complex' than those complexes in which the components are proximate. Exceptions occur, however, in the case of the fourth, fifth, and octave. It would seem that the qualitative disparity of these intervals is obscured by a greater covariation in clearness of the components, the factor which makes for 'simplicity.' That is to say, processes which ordinarily lead to the judgment 'complex,' are cut across in the case of the octave, fifth, and fourth by more insistent processes leading to the judgment less 'complex'—more 'simple.' It should be noted that in the characterizations of 'simplicity' all the *O*'s except one had occasion to speak of those intervals in which covariation in clearness made analysis difficult as "stuck together," "glued together," "well knit," "well fused," "hanging together," etc. Such characterizations of the impressions, together with the fact that covariation in clearness cuts across and obscures qualitative disparity, make plausible the statement that covariation in clearness is related to a true perceptual 'fusion' of tones, *i. e.*, fusion is an integration of attributes of clearness rather than a qualitative integration.

*Pleasantness and Unpleasantness.*—None of the *O*'s based their affective judgments upon *P* and *U per se*. In all cases the judgments were of the nature of attitudinal or 'meaning' responses. For two *O*'s *P* and *U* turn out to have their sensory bases in 'smoothness' and 'roughness.' For convenience of reference the affective preferences of these two *O*'s may be designated as judgments of 'smooth' *P* and *U*. The other three *O*'s, whose affective preferences accord more with musical usage, were unable to give any thoroughgoing characterization of the sensory mediators for *P* and *U*. In lieu of a better term their affective judgments may be referred to as 'musical' *P* and *U*.

*Volume.*—Two *O*'s found it possible to characterize the intervals in terms of 'voluminousness' or 'fulness.' It is evident from their descriptive reports that 'volume' for them is not a purely auditory affair, but an integration of auditory process with certain contexts which attach to the intervals. For one *O* 'volume' is an integration of auditory process and chest-kinaesthesia; for the other, an integration of auditory and visual processes.

*Horrisonorousness.*—By this term one *O* meant an intimate integration of auditory quality and ear-kinaesthesia into a closely knit pattern of vibrant qualitative roughness. Originally, in the preliminary series, this *O* noted that 'horrisonorousness' was frequently affectively toned. The agreement of this *O*'s affective preferences with certain of his judgments on 'horrisonorousness' makes it possible that the latter and the sensory basis of 'musical' *P* and *U* have something in common.

*Visual and Kinaesthetic Processes.*—Three *O*'s followed through series in which judgments were made upon concomitant non-auditory processes alone. Terms such as 'rotund,' 'soft,' and 'noisy' refer to the size, smooth contour, and jagged texture of concomitant visual processes; 'zusammenfliessend' to the pulling together or apart of visual imagery; and 'head-kinaesthesia' to the spread of kinaesthetic process in the posterior portion of the head. (See Chart 7.)

It is now possible, with the aid of the foregoing terms, which stand for definite sensory experiences ('musical' *P* and *U* excepted), to characterize every interval separately with respect to various factors in the total impression which contribute to its constitution.

*Octave.*—The octave occupies a unique position among the tonal intervals in that it lies at one extreme of three of the attributive continua within which all intervals vary. It is the 'smoothest,' 'simplest,' and least 'complex' interval, in the sense of the discussion of the preceding paragraphs. With respect to 'smooth' affective judgments the octave is also the most pleasant interval. *O*'s whose affective preferences are of the 'musical' kind, however, find the octave about as pleasant as the fourth and fifth and less pleasant than the sixths and thirds.

By some *O*'s the octave was also characterized as giving a more 'voluminous' impression than the other intervals.

*Fifth.*—This interval resembles the octave in that it is marked by an almost complete absence of 'roughness.' One *O*, in fact, judged the fifth as practically equal to the octave

in 'smoothness.' The components of the fifth admit of more independent variation in clearness than do the components of the octave, that is to say, the fifth is slightly less 'simple.' This factor of 'simplicity' tends to predominate over the wide 'sense-distance' included in the fifth, so that the interval appears as less 'complex' than any of the other intervals except the octave. Under a 'musical' affective determination the fifth is slightly less pleasant than the octave except for one *O* who finds it somewhat more pleasant than the octave. The fifth is also less pleasant than the octave for those *O*'s whose affective judgments are mediated by 'smoothness.'

The contextual aspect of 'volume' when reported was more prominent in the fifth than in any other interval except the octave.

*Fourth.*—With respect to 'smoothness' the fourth is on an equal plane with the sixths, which are a little less 'smooth' than the fifth. In this interval the 'simplicity' factor again appears to obscure the 'complexity' aspect, so that only the fifth and octave are judged less 'complex' than the fourth. The fourth, fifth, and minor sixth all appear about equally 'simple.' 'Musical' affective judgments place the fourth before the fifth and the octave in *P*; 'smooth' affective preferences place the octave and fifth before the fourth in *P*.

For *G*, who judged upon a qualitative pattern of integrated auditory process and ear-kinaesthesia, the fourth assumed a trace of 'horridness.' The 'voluminous' characteristic, when it was observed, entered into the fourth to the same extent as in the sixths, which are exceeded in 'volume' only by the fifth and octave.

*Minor Sixth.*—The limits which various characteristics may reach under attitudinal changes vary more between intermediacy and extremes in the minor sixth than in the three preceding intervals. The fourth and both the sixths are very 'smooth' intervals and are exceeded only by the fifth and octave in this respect. The remoteness of the pitch qualities in the minor sixth lends to the interval an aspect of 'complexity' which is more marked only in the major sixth and the sevenths. On the other hand, under an active attitude of attentional analysis, the relative lack of independent variability in clearness of the components leads to the judgment 'simple' in the case of three *O*'s, that is to say, perceptual 'fusion' is more insistent than qualitative disparity. 'Musically' the minor sixth is more pleasant than any of the



preceding intervals. When mediated by 'smoothness,' however, it is less pleasant than the preceding intervals.

For two *O*'s, only the octave and fifth exceeded the minor sixth with respect to 'voluminousness.' G found 'horrisonorousness' present in this interval to the same extent as in the fourth. For certain *O*'s contextual and kinaesthetic processes became palpable in the minor sixth to an extent which would tend, under an equivocal determination, to obscure the purely auditory aspects of the impression.

*Major Sixth.*—The qualitative disparity of the components of the major sixth is even more marked than in the minor sixth, giving thus to the interval an aspect of 'complexity' exceeded only by the sevenths. Under the determination for 'simplicity,' however, three *O*'s find but little difference in 'simplicity' between the major sixth and the minor sixth and fourth. The major sixth is on equal terms with the minor sixth and fourth with respect to 'smoothness.' Only the thirds are pleasanter than the major sixth under a 'musical' affective determination. The other affective judgments place the major sixth just below the preceding intervals in *P*.

'Volume' was somewhat less and 'horrisonorousness' slightly more apparent in the major sixth than in the preceding intervals, for the *O*'s who found these contexts. As in the minor sixth, kinaesthetic and visual processes may have come in to obscure the auditory processes in the major sixth.

*Major Third.*—The relative position with respect to the other intervals of the major third, and also the minor third, varies considerably under attitudinal changes. Between the major third and the intervals already mentioned there is a marked difference in 'smoothness.' Only the minor third and the seconds are 'rougher' than the major third. Only the octave, fifth, fourth, and seconds are less 'complex,' and the octave, fifth, fourth, and sixth more 'simple,' than the major third. The relative 'simplicity' and 'non-complexity' of this interval are presumably not due so much to the covariation of clearness and the parity of pitch qualities of the components as to the obliteration of these processes by the rapid intensive variations of the temporal 'roughness' pattern. The major third is the most pleasant of all the intervals for those *O*'s whose affective judgments have a 'musical' mediation, whereas it is "indifferently" toned for the two other *O*'s.

The auditory-kinaesthetic context of 'horrisonorousness' was very prominent in this interval for G. For F and Y the 'voluminous' context of the major third was almost lacking. The auditory factors of the major third may also have been



considerably obscured by visual and kinaesthetic concomitant processes.

*Minor Third.*—With the exception of the preceding interval, the minor third is the most pleasant interval for those *O*'s whose affective judgments are of the 'musical' sort. The interval is rather unpleasant for those *O*'s whose affective preferences are mediated by 'smoothness.' One of the most striking characteristics of this interval is the prominence of the factor of 'roughness.' Only the seconds, and for two *O*'s the major seventh, are 'rougher' than the minor third. As with the major third, the temporal-intensive pattern of 'roughness' seems to blot out the separate auditory qualities with the result that the minor third is judged as relatively 'non-complex.' Three *O*'s find the minor third, however, less 'simple' than any of the preceding intervals because of a shift to a 'meaning' attitude in the presence of the 'rough' intervals; that is to say, intervals in which oscillations of intensity are very prominent are cognized as 'non-simple' by virtue of this very factor of 'roughness.'

'Horrisonorousness' was more prominent in the minor third than in any other interval for G. Concomitant non-auditory processes manifested themselves to a great extent in the minor third.

*Tritone.*—In none of the aspects which enter into tonal impressions does the tritone seem to occupy an extreme position with respect to the other intervals. Perhaps the most striking characteristic of the interval is its 'smoothness.' Only the group of intervals comprising the octave, fifth, fourth, and sixths, is smoother than the tritone. The qualitative disparity of its components is sufficiently marked to cause the interval to be judged as more 'complex' than the octave, fifth, fourth, thirds, and seconds, though not so 'complex' as the sixths and sevenths. For three of the *O*'s the tritone appears somewhat more 'simple' than the thirds. For all the *O*'s the tritone is "indifferently" affective.

G found 'horrisonorousness' rather well marked in the tritone, although not to so great an extent as in the thirds.

*Minor Seventh.*—The 'roughness' of the minor seventh is not very obvious, although, of course, the interval is not so 'smooth' as the tritone and the group of intervals already mentioned. It is about equal in 'roughness' to the major third. The qualitative disparity in pitch of the components of the minor seventh lends to the interval an aspect of 'complexity' only exceeded by the major seventh. Three *O*'s under a determination for analysability, however, find the minor

seventh rather 'simple,' *i. e.*, the independent variability in clearness of its components is not very marked. The interval is unpleasant for *O*'s whose affective judgments are mediated by 'smoothness' as well as for those whose judgments are 'musically' mediated, although less pleasant for the latter than the former.

*O*'s, who judged on the basis of 'volume,' found the minor seventh very 'voluminous.' Other contextual processes were also apparent to a noticeable extent for some *O*'s.

*Major Seventh.*—With the exception of the seconds, and for three *O*'s the minor third, the major seventh is the 'roughest' interval. It is also the most 'complex' interval. Only the seconds, when cognized as 'non-simple' under a 'meaning' attitude, are judged less 'simple' than the major seventh. The interval is very unpleasant for all *O*'s. For the 'musical' type it is the most unpleasant, while for the 'smooth' type the seconds are somewhat more unpleasant. It can be seen that the extreme position which the major seventh occupies within certain continua tends to give it a unique status antipodal to the octave.

Only the thirds exhibited greater 'horrisonorousness' than the major seventh. For some *O*'s visual and kinaesthetic processes and the contextual 'voluminous' aspect entered very prominently into this interval.

*Major Second.*—Unquestionably the most striking feature of the major second lies in the violent intensive oscillations constituting the 'roughness' pattern. Only the minor second possesses this characteristic in a more pronounced degree. For all *O*'s the major second is very unpleasant. Under a 'meaning' attitude the seconds are cognized as the least 'simple' intervals; but under a strictly descriptive attitude they scarcely submit to psychological separation at all, *i. e.*, the auditory qualities can not be singled out in the face of the violent intensive variations which dominate the seconds to so great an extent.

The contextual factor of 'volume' dropped out in the seconds, as did also 'horrisonorousness.' The interval was rich, however, in kinaesthetic and visual processes.

*Minor Second.*—This interval is the 'roughest' of all. Under an affective determination of the 'smooth' type it is the most unpleasant interval. With respect to other aspects the minor second is similar to the major second: under a 'meaning' attitude an *O* knows that a violently 'rough' interval must be 'non-simple;' but under a descriptive attitude he

judges it as 'non-complex,' since the component qualities can not be separated out.

From the charts and the immediately preceding characterizations of the intervals it can be seen that the rank-order of certain bitonal complexes is more affected by change of attitude than is the rank-order of others. The thirds and sixths, *e. g.*, undergo greater alteration in position under varying instructional determinants than do the octave, fifth, and fourth. The seconds shift position very decidedly in the presence of a descriptive attitude as against a 'meaning' attitude; but since these differences of attitude are of a different order from those brought about by the more readily effective instructional determinants, the variation of the seconds may be left out of account in the present consideration. In the literature upon tonal fusions there is considerable agreement regarding the ranks of the octave, fifth, fourth, and seconds;<sup>25</sup> but the sixths, thirds, and sometimes the minor seventh, are variously placed by different experiments. In the present experiment the results of the 'unitariness' series (Chart 1) show a like agreement among *O*'s of the octave, fifth, fourth, tritone, sevenths, and seconds, and a considerable divergence for the thirds and sixths. The preceding discussion has shown that the sixths and thirds vary in rank more than the other intervals under change of instructional determinant. The difference becomes apparent if the averages of all the intervals of all the series, except the 'unitariness' series, are taken and their *M. V.*'s computed, and the intervals arranged in order in terms of decreasing size of *M. V.* This order is as follows: minor third, major sixth, minor sixth, major third, minor seventh, major seventh, octave, fifth, tritone, fourth, and seconds. It seems to indicate that the possibility of equivocal determination of judgment is greater for the sixths and thirds, and occasionally for the sevenths, than for the other intervals. This fact may explain the lack of agreement in the literature concerning the rank-order of these particular intervals, since the instructional limitation of an *O* to a particular sensory criterion is extremely difficult at best, and, in the absence of a previous knowledge of the phenomenological criteria available in tonal intervals, almost impossible.

<sup>25</sup> See *e.g.*, R. Schulze, Ueber Klanganalyse, *Philos. Stud.*, 1898, 14, 483; Stumpf, *Tonpsychologie*, 1890, ii, 135; A. Meinong and St. Witasek, Zur experimentellen Bestimmung der Tonverschmelzungsgrade, *Zeits. f. Psychol.*, 1897, 15, 191, 193; A. Faist, *ibid.*, 109-113; Kemp, *op. cit.*, 186-190; Pear, *op. cit.*, 66.

## ON ARTERIAL EXPANSION

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Such a great deal of experimentation has been published on plethysmographic work that some time ago Leschke<sup>1</sup> attempted to reach some conclusions by submitting the results of all these studies to a statistical treatment. All of this work, however, has been qualitative; the authors have sought to determine whether the volume of the blood at the periphery increased or decreased. We are unable to find any study in which the amount of increase or decrease has been quantitatively studied. Such a quantitative determination is the object of the present study.

A Lehmann plethysmograph was employed to the glass tube of which an arbitrary scale was attached. The volume of the body of the plethysmograph and of each division of the scale was determined. When the subject was placed in the instrument, the water was taken from a large graduated tube. The volume of the arm was thus determined by subtracting the volume of the water used from the total volume of the plethysmograph.

Three series of determinations were made on each of 24 adult subjects. (1) A Stanton sphygmomanometer was placed above the elbow of the arm used. The sac was then inflated until the diastolic pressure was obtained. In this case the veins were closed while the arteries remained open, and hence the blood backed up in the forearm expanding the arteries to the limits of their elasticity. After this determination the sphygmomanometer was removed. (2) The subject was then instructed to hold his breath for as long a time as possible and a second determination was made. (3) The third determination was made during mental work; mental multiplication of a three-digit by a two-digit number being sufficient, in most cases, to keep the subject concentrated for a period of several minutes. At the beginning of each determination, the reading of the height of the column of water in the tube was recorded, as was also the *greatest* extent of change during the experiment.

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<sup>1</sup> Leschke, E., Die körperlichen Begleiterscheinungen seelischer Vorgänge, *Arch. f. d. ges. Psychol.*, 21, 1911, 435ff. Die Ergebnisse und die Fehlerquellen der bisherigen Untersuchungen über die körperlichen Begleiterscheinungen seelischer Vorgänge, *ibid.*, 31, 1914, 27ff.



Sub- ject	Volume of Arm in C. C.	Volume Change Diastolic in C. C.	Volume of Change in C. C. Holding Breath	Volume of Change in C. C. Mental Work	% of Change Diastolic	% of Change Holding Breath	% of Change Mental Work
1	708	12.5	0.0	0.0	1.77	0.00	0.00
2	732	7.5	-1.2	-2.5	1.02	0.17	0.34
3	1103	17.5	0.0	0.0	1.59	0.00	0.00
4	840	5.0	-5.0	-1.2	0.60	0.60	0.15
5	845	15.0	-1.2	-1.2	1.77	0.15	0.15
6	808	10.0	0.0	-1.9	1.24	0.00	0.23
7	888	16.2	0.0	1.2	1.83	0.00	0.14
8	1111	8.8	-3.1	0.0	0.79	0.28	0.00
9	1400	17.5	-3.8	-2.5	1.25	0.27	0.18
10	773	5.0	-1.2	-1.2	0.65	0.16	0.16
11	830	21.2	-7.5	-1.2	2.60	0.91	0.15
12	810	11.2	-2.5	-1.2	1.40	0.31	0.16
13	985	45.0	-1.2	-2.5	3.17	1.25	0.17
14	782	15.0	1.2	1.2	1.90	0.16	0.16
15	788	12.5	0.0	-1.2	1.60	0.00	0.16
16	908	22.5	-1.2	-2.5	2.48	0.14	0.28
17	1022	17.5	-0.6	1.2	1.70	0.06	0.12
18	812	7.5	-0.6	-1.2	0.92	0.01	0.15
19	1045	16.2	-1.2	0.6	1.56	0.12	0.06
20	838	22.5	-2.5	-2.5	2.68	0.29	0.29
21	955	20.0	-2.5	-3.7	2.01	0.25	0.38
22	1030	37.5	10.0	-1.2	2.75	0.67	0.08
23	1078	30.0	-2.5	1.2	2.78	0.23	0.12
24	1142	25.0	2.5	3.7	2.19	0.22	0.03
Ave.	926	17.9	2.2	1.5	1.76	0.26	0.16



The results are found in the accompanying table. In the first column are indicated the subjects by number. In the next column the volume of the arm under normal conditions is recorded. In the next three columns are the volumes of change in c. c. respectively under the conditions of diastolic pressure, of holding the breath, and of mental work. A *minus*-sign indicates a decrease in volume. Inasmuch as the volume of the arm varies considerably for the different subjects, the actual amount of increase or decrease loses in significance when the different subjects are compared. Hence the percentage of change was calculated for the different experimental situations. These values are found in the last three columns of the table. The averages for the values in each column are found in the bottom row.

It will be noticed that the changes under the conditions of diastolic pressure are very much larger, on the average, than those for either mental work or holding the breath. The average for diastolic pressure is 1.76 *per cent.*, the greatest change being 3.17 *per cent.* (Subject 13), the smallest 0.60 *per cent.* (Subject 4). In the case of holding the breath, the average change is 0.26 *per cent.*, the greatest being 1.25 *per cent.* (Subject 13); while for Subjects 1, 3, 6, 7, and 15 no change was noted. For mental work, the average change was 0.16 *per cent.*, the greatest change being 0.38 *per cent.*; while for Subjects 1, 3 and 8 no change was noted. The change for diastolic pressure was greater than that for holding the breath in every case but one (Subject 4) where it was equal but in the negative direction. The change for diastolic pressure was invariably greater than for mental work. In 10 cases the change for holding the breath was greater than for mental work, in 6 cases it was equal, and in 8 cases less.

#### SUMMARY

- (1) This study suggests a method for dealing with volumetric peripheral vascular changes in a quantitative way.
- (2) Under our conditions we were unable to get increases in volume of blood in the forearm which approached the limits of elasticity of the vascular system.
- (3) The combination of the sphygmomanometer with the plethysmograph appears to give a better determination of arterial elasticity than does the present medical clinical method.

## FUNCTIONAL PSYCHOLOGY AND THE PSYCHOLOGY OF ACT: I<sup>1</sup>

By E. B. TITCHENER

§ 1. In the dichotomy by Extension, physical phenomena receive positive and psychical phenomena receive negative determination. In another dichotomy, which is perhaps even more familiar, this relation is reversed: the universe of experience is divided into the Conscious and the Not-conscious, and psychical are identified with conscious phenomena, mind with the totality of consciousness. Psychology thus receives at length a positive definition.<sup>2</sup>

The word 'consciousness' is, however, notoriously ambiguous;<sup>3</sup> and the question whether the adjective 'conscious' suffices to mark off a special class of phenomena must therefore be discussed with great care. It must, moreover, be discussed in two separate contexts. For the psychologists who recognise the independent existence of 'phenomena of consciousness,' as the given objects of psychological investigation, fall at the present time into two principal groups. The one of these emphasises, in a psychology of 'function,' the biological aspect of empirical psychology; the other, in a psychology of 'act,' emphasises its intentional aspect.<sup>4</sup> Both alike may

<sup>1</sup> In writing a chapter on "The Definition of Psychology: Subject-Matter," for the introductory volume of a Systematic Psychology, I have been obliged to take critical account of functional psychology and the psychology of act. Since it seems fruitless to publish conclusions without giving the evidence on which they rest, and since at the same time a book such as I have planned is not the place for these criticisms of detail, I print my comments in the JOURNAL. The present article deals with functional psychology; a following article will deal with the psychology of act. The first section of the text follows a discussion of the definition of mind, negatively, by "absence of extension" (Bain).

<sup>2</sup> Moreover, the Not-conscious may be further subdivided, in the interest of biology, into a Living and a Not-living. There can be no doubt that, formally regarded, this classification is superior to that by Extension.

<sup>3</sup> The *locus classicus* is A. Bain, *The Emotions and the Will*, 1880, 539ff. Cf. also R. Eisler, *Wörterbuch der philosophischen Begriffe*, i., 1910, 177ff.

<sup>4</sup> Functional psychology, in this sense, is especially American, and the psychology of act especially German. Recent English psychology, through G. F. Stout (*Anal. Psychol.*, i., 1896, 36, 40), has been influenced by Brentano.

therefore trace their descent from Aristotle.<sup>5</sup> But in spite of much that they hold in common, the difference of motivation makes it necessary to consider the two groups separately.

In dealing with 'functional' psychology, we shall first examine a single representative system, and shall then bring together, for critical review, what appear to be the major tenets of the school. Systems that are based upon the concept of consciousness, even if this be taken under a single aspect, will inevitably differ; and we find accordingly that the biological emphasis falls differently in different functional psychologies. For Ladd, the 'stream of consciousness' is a life, and mind, the totality of consciousness, is an organism, "a unique and living totality in a course of development,"—an organism living a life of its own, which is always connected with the bodily life, but yet is "in some sort independent of" the body; and functional psychology is the science of the functions or activities of this psychical organism.<sup>6</sup> Angell, on the other hand, regards consciousness as an organic function,<sup>7</sup> a phenomenon of control;<sup>8</sup> "mind," he declares, "seems to involve the master devices" whereby the "adaptive operations of organic life may be made most perfect."<sup>9</sup> Judd, again, appears to combine both views. At first, consciousness is represented as an organic function, as something which, like the digestive or locomotive function, plays its part in the economy of the organic life under the conditions of the biological struggle for existence; it is thus one of the 'attributes' of man, considered as the highest animal.<sup>10</sup> Later, consciousness is 'possessed' by a knowing self, an unitary being which

<sup>5</sup> Intentionalism is ascribed to Aristotle on the ground of his doctrine of the relation of sensation to object of sense (*De anima*, 424a, 425b), and of thought to the object of thought (*ibid.*, 429a, 430a, 431b). Cf. also his statement that the objects of memory and imagination (*De mem.*, 450a) and those of thought and desire (*De an.*, 433a; *Met.*, 1072a) are identical.

<sup>6</sup> G. T. Ladd, *Psychology, Descriptive and Explanatory*, 1894, 638, 659ff.; *Philos. of Mind*, 1895, 400ff., esp. 405; G. T. Ladd and R. S. Woodworth, *Elements of Physiol. Psychol.*, 1911, 656ff.

<sup>7</sup> J. R. Angell, *Psychology*, 1904, 79. In 1908, 95 the phrase is dropped, but the meaning of the passage appears to remain the same.

<sup>8</sup> Angell, "The Province of Functional Psychology," *Psychol. Rev.*, xiv., 1907, 88.

<sup>9</sup> *Psychol.*, 1908, 8. In 1904, 7, "mind seems to be the master device." Cf. also 1904, 50, 86 with 1908, 64, 103. In both editions mind is "an engine for accomplishing the most remarkable adjustments of the organism to its life conditions:" 1904, 379; 1908, 436.

<sup>10</sup> C. H. Judd, *Psychology*, 1917, 4, 161; "Evolution and Consciousness," *Psychol. Rev.*, xvii., 1910, 84.

finds its nearest analogy in the living organism.<sup>11</sup> Differences of this sort find their natural expression in the classifications and arrangements and relative emphases of the writers' systematic works. But they have little if any bearing on the question immediately before us; they do not destroy the essential unity of the functional school.

The psychology of 'act' cannot be treated in the same comprehensive way. For here, as we quickly recognise, individual differences are not only inevitable but also fundamental and constitutive, so that the principal systems and programmes must be separately considered. Brentano, of course, furnishes both our starting-point and our constant point of reference. Besides Brentano, we pass under review, either in their own person or in that of some member of their school, Meinong, Stumpf, Lipps, Husserl and Külpe. We then interrupt the course of the exposition, in order to compare and contrast two experimental text-books of intentionalistic psychology, Witasek's *Grundlinien der Psychologie* (1908), which systematises Meinong's views, and Messer's *Psychologie* (1914), which we may regard as, in large measure, a systematisation of the later views of Külpe. The digression will be useful: it will reveal likeness and difference, in kind and in degree, and will thus prepare us for a broader survey of the act-systems. But here, we repeat, it proves impossible to follow the lines laid down by our study of functional psychology. We shall rather enquire, first, into the significance for these systems of the classification of psychical phenomena, and secondly into their treatment of two modes of such phenomena, namely, sensation and attention. The choice of topics is not arbitrary. It is suggested by the foregoing review of the principal systems; and it leads us to certain general conclusions with respect to intentionalism at large.

#### *Ladd's Systematic Psychology*

§ 2. We may take, as broadly typical of the functional systems, Ladd's definition of psychology: "the science which describes and explains the phenomena of consciousness, as such."<sup>12</sup> Consciousness, Ladd says, cannot in strictness be defined. Its meaning may, however, be brought out by contrast. "What we are when we are awake, as contrasted with what we are when we sink into a profound and perfectly dreamless sleep, or receive an overpowering blow upon the head—that it is to be conscious. What we are less and less

<sup>11</sup> *Psychology*, 274ff. Cf. below, p. 532.

<sup>12</sup> G. T. Ladd, *Psychology, Descriptive and Explanatory*, 1894, 1.

phenomenon of consciousness' is, as we saw, simply conscious, and cannot become increasingly or decreasingly what it essentially is.

We are, however, not yet at the end of Ladd's account. The state of consciousness which is to be the subject-matter of psychology must, he points out, do more than merely exist; it must become an 'object of knowledge.'<sup>23</sup> But it can become an object of immediate knowledge only by way of introspection or self-awareness;<sup>24</sup> and since this observing activity is itself a phenomenon of consciousness, the total state of consciousness, as object of psychological knowledge, is not just awareness, but rather self-awareness, awareness of awareness. The state remains unitary. If, however, we have recourse to logical abstraction, then the second awareness, the "phenomenon [of consciousness] known as fact," may be distinguished as 'content' from the activity of observation, the "knowing of the phenomenon [of consciousness] as object."<sup>25</sup> In this way the observer is brought within the conscious field, and degrees of consciousness are so far justified.

Still we are not at an end. The 'content' thus abstractly marked off from the activity of consciousness remains consciousness, and must therefore in its turn admit of the same distinction of activity and passivity. Sensations, ideas, feelings, conations may be regarded, passively, as 'content' of consciousness.<sup>26</sup> Their description and explanation make up half of the detailed psychological story. In the other half, consciousness (conscious content in the broader sense) is regarded actively: as intellection (or awareness of likeness and difference), as reactive feeling (mental tension, conviction), as attention.<sup>27</sup> Ladd is emphatic that "the task of a scientific psychology is as truly the description and explanation of the phenomena of consciousness, considered as forms of active functioning (of consciousness 'function-wise'), as it is the description and explanation of the particular qualities and quantities of the phenomena regarded as passive states (of consciousness 'content-wise')."<sup>28</sup> And he expressly applies this dictum to the "single state of consciousness, so far as

<sup>23</sup> *Ibid.*, 1f., 4, 7, 9, 32, etc.

<sup>24</sup> *Ibid.*, 9, 15, 523, etc. Introspection is also called self-consciousness and reflective consciousness.

<sup>25</sup> *Ibid.*, 32f.; cf. 37, 49, 289ff. Ladd varies in his use of the terms 'active' and 'passive:' see 46, 83, 96, 214, etc.

<sup>26</sup> *Ibid.*, 309. There seems to be inconsistency as regards feeling: cf. 19f, 163, 523.

<sup>27</sup> *Ibid.*, 288ff.; 308f.; 213, 289.

<sup>28</sup> *Philos. of Mind*, 86.



we can catch it and separate it from the stream of conscious life," *i. e.*, to what he has called the phenomenon known as fact, no less than to the "stream of conscious life in which every such state occurs," *i. e.*, to the total consciousness which includes the knowing along with the known.<sup>29</sup>

It appears, then, that Ladd operates with two distinct notions of 'consciousness': the notion of consciousness as the sum-total of conscious states which make up the experience of an 'I,' and the notion of consciousness as the observing activity of this 'I' itself. If the states alone are conscious, there should be no more or less of consciousness: but Ladd, assimilating the conscious character of the states, their essential nature as awarenesses, to the conscious character of the observing self, is able to speak of degrees of consciousness at large. The complete datum of psychology, awareness of awareness, he regards as intrinsically unitary, but as separable by logical abstraction into awareness knowing and awareness known, activity or function and content of consciousness. The awareness known, the content-awareness, he then divides again, by the same abstraction and in the same terms, into activity or function and content. These divisions are confessedly artificial: there is no real line of division within the psychical fact: and we must suppose—since there seems to be no logical reason why they should not be repeated *ad infinitum*<sup>30</sup>—that they are carried only so far as is necessary for systematic exposition. Ladd himself justifies them, partly on the ground of convenience, but also, in the general portion of his treatise, because they indicate that psychology, the science of the phenomena of consciousness as such, is far more than description and explanation of merely passive 'content.'<sup>31</sup>

§ 3. In seeking to appraise Ladd's definition of the psychical, as subject-matter for a science, we shall confine ourselves to a few broad lines of criticism. And we begin by consider-

<sup>29</sup> *Psychol.*, 290f.

<sup>30</sup> Brentano (*PES*, i., 1894, 167) avoids the infinite regress by means of his *eigenthümliche Verwebung*: cf. my *Exper. Psychol. of the Thought-processes*, 1909, 47f. Ladd (*Psychol.*, 35) comments critically on Brentano's fourfold act, but does not appear to recognize the logical difficulty in which he is himself involved. I do not know that every logical regress is necessarily vicious. But in this case Ladd's system is not intelligible without the regress, while the nature of the regress itself is (as it seems to me) to be unintelligible.

<sup>31</sup> It follows, of course, that Ladd's 'physiological psychology' is, as psychology, only a portion of a science, and not an organized scientific whole: see Ladd and Woodworth, *Physiol. Psychol.* 381, 430, 542, 597, 625, 656, 664f.

ing (1) the place or position to which the definition assigns psychology within the group of the acknowledged sciences.

Observation, the immediate awareness of fact, seems in Ladd's view to be identical over the whole range of science. He draws no distinction of kind between inspection and introspection.<sup>32</sup> When, however, we turn to the relation between this direct method of acquaintance and the object upon which it is directed, we find that psychology is "peculiar, and indeed unique." For observation, being itself a fact of consciousness, merges into, or fuses with, the facts of consciousness observed; "it is separable, neither in reality nor in time, from the phenomenon observed as fact."<sup>33</sup> Our own discussion of psychological method must be postponed. Here we note only that, as regards the relation of its primary method to its subject-matter, Ladd marks off psychology, as unique, from all the other sciences.

The subject-matter of psychology, in what (for want of a better phrase) we may call its logical constitution, is also, for Ladd, unique. The state of consciousness, it will be remembered, is always, at one and the same time, fact of knowledge, fact of feeling and fact of conation. "This unity in variety, which belongs to all states of consciousness as such, is of unique character—and this, whether we lay emphasis on the unity that comprises the variety, or upon the variety comprised in the unity."<sup>34</sup> This uniqueness of constitution, exemplified by the single state of consciousness, is attributed by Ladd to the entire course of the mental life.<sup>35</sup>

Not only in logical constitution, however, but also in its self-determination, is the subject-matter of psychology unique. Descriptive and explanatory psychology leads us "to recognise a unique and self-active being" as, within limits, "interiorly

<sup>32</sup> *Psychol.*, esp. 17f. Even the fact that psychical phenomena are "alterable—swiftly and largely—by the very act of attention which makes them objects of knowledge" seems to be paralleled on the side of inspection: see 18 (§ 3), 305f., 318f., 367f., etc.

<sup>33</sup> *Ibid.*, 32f., 319, 530, etc. Cf. *Philos. of Mind*, 160: "The knowing subject and the object are . . . woven into a vital oneness of being."

<sup>34</sup> *Psychol.*, 36, 172f.

<sup>35</sup> There is, namely, a "principle of continuity which gives its unique character to what we can observe of mental development. In all forms of organic physical evolution . . . the factors and stages of the evolution have some existence and value considered *in themselves*, as it were. But the case of mental development is not so. . . . Each factor, faculty, and stage exists for consciousness as in and of its own continuously flowing life-movement;" *ibid.*, 659f. The unity and variety of states of consciousness "are illustrations of this very principle of continuity as lying at the base of mental development:" 661.

determining, in a quite inexplicable way, its own course."<sup>36</sup> The presence among its data of this incalculable surd—whatever the limits of its operation may be—again separates psychology from the other sciences.

Here, then, are three points at which scientific psychology is wholly and necessarily out of touch with what should be its fellow-sciences. It is clear that the word 'science,' if it is to be retained at all, must be given an extraordinarily elastic meaning. In fact, the position which Ladd assigns to scientific psychology is a position, not within, but without the circle of the acknowledged sciences.<sup>37</sup>

(2) Ladd, however, has his own definition of science. "There is science," he says, "wherever there are ascertainable facts that may be described and explained in their relation to one another and to other classes of facts."<sup>38</sup> We must therefore consider, secondly, the nature of the 'facts of consciousness' that appear in his pages as the data of a scientific psychology.

Throughout his psychological writings Ladd is insistent that the phenomena of consciousness be regarded both content-wise, as facts of passive existence, and function-wise, as facts of activity.<sup>39</sup> The task that he sets himself is, accordingly, twofold: he must describe and explain both the content and the function of every typical psychosis; and, indeed, he must describe and explain the particular content as adequate vehicle of a particular function, and the particular function as correlated activity of a particular content. This of itself would

<sup>36</sup> *Ibid.*, 638, 662. We might append, as a fourth point, the fact that psychology sustains a "quite unique" relation to philosophy: *ibid.*, 638; *Philos. of Mind*, 71.

<sup>37</sup> Ladd can be sufficiently severe upon these 'acknowledged' sciences: e.g., *Philos. of Mind*, 6ff. The point here, however, is that he distinguishes psychology by its three (or four) unique characters from the other sciences, not as seen by themselves, but as he sees them.

<sup>38</sup> *Psychol.*, 658. The 'affirm' of the following sentence should apparently be read 'deny.'

<sup>39</sup> Ladd observes that "a psychosis without content is equivalent to no psychosis at all; there are no phenomena of consciousness in general" (*Philos. of Mind*, 85; cf. *Philos. of Knowledge*, 200, and *Psychol.*, 30f.). Yet he frequently speaks, in the *Psychology*, as if consciousness were, after all, something 'apart from' and superadded to the 'actual psychic facts'; we read of 'conscious psychic activity' (214), 'conscious intellection' and 'conscious mentality' (296), 'the conscious mind' (305), 'conscious mental life' (321, 469), 'conscious feeling' (583, 587), 'conscious ideation' (600), 'conscious conation' (623), 'conscious acts of will' (657), and 'conscious attention' (666f.). Similar phrases occur in Ladd and Woodworth, *Physiol. Psychol.*, 380, 463, 642, 671, 676, 679. These slips give further evidence of the unstable nature of Ladd's concept of consciousness.



be a sufficiently difficult programme; but Ladd is hampered in his undertaking by his double use (to which we have already referred) of the term 'consciousness,' which means both 'my' awareness and also the awareness intrinsic to a psychosis as such. Consider, for example, that "convenient abstraction," the sensation. Every sensation is at once active and passive, a "psychical activity" and a passively received "impression." As active, it should be actively aware, actively cognisant of the "quality belonging to the object of sense." In Ladd's account, however, it becomes, even while regarded as intrinsically active, an "item of information" to 'me;' 'my' sensations "become objectified, as my feelings and thoughts cannot, in the form of qualities of perceived *things*." The function which should be the sensation's own is thus transferred, from the sensation, to the 'me' to whom all sensations belong.<sup>40</sup>

In this case, then, there is loss to be noted; in the case of primary intellection, on the other hand, we have a superfluity. Intellection, as actively discriminating consciousness, is "within," is "an integral part of," every state of consciousness, so that a concrete psychosis is by its very nature self-discriminating and self-discriminated. Yet if I make a state of consciousness the object of my regard, discriminating consciousness is found to "accompany" the now passive fact. It is perhaps intelligible that 'my' discriminating consciousness should be needed to discern the discriminating activity which is native to the psychosis (though it must be remembered that, as such, these two activities are identical); but it is surely not clear how this attendant discrimination of mine helps toward the discernment of the already self-discriminated 'content.' Two discriminating activities, of precisely the same kind, are here set to work in circumstances where it seems that one would be sufficient.<sup>41</sup>

In still other cases, the duplication of consciousness leads to sheer logical confusion. Suppose, for instance, that I, by way of primary attention, make a state of consciousness the passive object of my consideration: primary attention is then the degree of psychical energy "expended upon" the different aspects or moments of the state. Yet every mental state, as active, has its own degree of this same psychical energy, upon which attention, in its present turn, is constantly dependent. Ladd tries to save his logic by the remark that these two statements "only serve to approach the same truth from different

<sup>40</sup> *Psychol.*, 93ff.

<sup>41</sup> *Ibid.*, 33f., 288.

sides." But the truth, as he leaves it, seems rather to be a matter of alternatives.<sup>42</sup>

Difficulties of this sort recur again and again to baffle the student of Ladd's psychology. The 'facts of consciousness' with which the system deals are both ambiguous and instable. They are active and they are passive, they are forthputtings of a mind and data of a science, they are conscious and I am conscious of them. Moreover, they play their systematic parts with so little regard to omission and repetition and contradiction that the plot of the play tends to be lost.

(3) This plot itself, we must add,—the systematic working up of the psychological materials,—shows a like ambiguity. Ladd declares, for example, that the mental life, from its very beginnings, carries the plain promise of a plan, so that "no science of the life of mind is possible without recognising the presence of final purpose;" yet he affirms also that "in attempting a scientific account of the mental life psychology is justified in laying emphasis, at first, upon the passive and, as it were, externally determined side of the total development," as if no plan were visible or operative.<sup>43</sup> Here is no distinction of function and content within consciousness, but the recognition of two radically different psychological attitudes toward mind in general. The same attempt to have things both ways is seen in the treatment of the faculties: Ladd, in all literalness, both accepts and rejects the doctrine of faculties. He speaks, in formal reference, of the 'so-called' faculties; he explains that the term 'faculty' is both futile and dangerous; and still he employs the concept, repeatedly and constructively, in his psychological exposition.<sup>44</sup> And, as a final instance, the same criticism holds of Ladd's treatment of mental composition. He makes free use of the terms 'mix-

<sup>42</sup> *Ibid.*, 74f., 78, 83, 621. On 'psychic energy' (of which Ladd gives no definition) see 39, 41, 44, 64f., 78, 83, 132, 261, 386, etc.

<sup>43</sup> *Ibid.*, 266f., 286f., 414, 664f., 668; *Philos. of Mind*, 203; *Philos. of Knowledge*, 473, etc.

<sup>44</sup> *Psychol.*, 33, 45f., 49ff., 60, 288, 317f. 380, 409f., 455, 490, 612, 659, 664. In particular: (1) Ch. iv. is headed "The So-called 'Mental Faculties.'" Ladd's use of 'so-called' is a mannerism, which began perhaps as a defensive reaction against the possible charge of looseness of language. Since it has come with him to mean anything from 'what is ordinarily called' to 'what is falsely called,' it does him little service. (2) The adjectives 'futile' and 'dangerous' are not too strong. For we are told (51) that words like 'faculty' do not explain; that they help but little in classification; and that their use, however guarded, is "likely to occasion. . . a generally inadequate and misleading account of the development of mental life." (3) For the seriousness with which the concept is taken, see such passages as 380, 490.



ture' and 'blending' and 'fusion' and 'association,' while yet he assures us that this language is figurative, adopted only for the avoidance of "almost unending periphrases." But is taste, or is it not, a complex of "gustatory, olfactory and tactual elements?" And is there, or is there not, in stereoscopic vision, a 'combination' of visual with tactual and motor sense-complexes? The reader looks in vain for a single periphrasis to clear his mind, and is forced to the conclusion that Ladd's logic is trying to say both Yea and Nay of the same subject-matter.<sup>45</sup>

(4) Where both data and applied logic are thus ambiguous, it is perhaps unnecessary to urge that the resultant is not an organised system. Since, however, we are presently to discuss the working concepts of 'function' and 'content' in their general applicability to the subject-matter of psychology, we cannot afford to neglect this formal point of criticism. We note briefly that Ladd's system breaks bounds on every side. Ladd starts out with the threefold unity of intellection, feeling and conation, all of which 'moments' or 'aspects' of consciousness are to be taken both as content and as function. These are the material postulates of his psychological system. But they prove to be inadequate. The study of intellection, for instance, brings us in time to the problem of recognitive memory; and here our scientific advance is arrested; recognition is "a form of mental reaction *sui generis*, which, while depending upon conditions . . . , has still a unique character that transcends the conditions on which it reposes."<sup>46</sup> In like manner the study of feeling brings us to the feeling of obligation and the sentiment of moral approbation or disapprobation. "These two forms of moral feeling are unique. Why they arise in the individual, and why they have that nature and connection with each other, and with the development of intellect, which they actually have—these are questions which psychology [even as explanatory] cannot answer."<sup>47</sup> Lastly, the study of conation brings us to volition, where "psychologically considered, it is no less true that I will the influential ideas, feelings and desires, than that the ideas, feelings and desires influence the final 'I will.'"<sup>48</sup> So that there

<sup>45</sup> The following sentence (*ibid.*, 235) is characteristic: "The introspective and experimental analysis of modern psychology cannot be abandoned, because, in spite of repeated explanations, some readers will persist in misunderstanding our necessarily figurative terms." Cf. 18f., 23, 37, 38f., 89ff., 94, 102f., 106, 115, 118, 132, 141ff., 146, 160, 180, 186, 209, 253ff., 318f., 323, 349, etc.

<sup>46</sup> *Ibid.*, 382, 397, 399, 401.

<sup>47</sup> *Ibid.*, 581ff.

<sup>48</sup> *Ibid.*, 618f., 625f., 635, 638.

is no single aspect of mind, no single strand of mental development, for which the complication of function and the compounding of content are sufficient. The system is disrupted on the side of intellect, on the side of feeling, and on the side of will.—

There can be little question, then, that Ladd has failed to erect a science of psychology on the basis of a special class or department of "phenomena of consciousness." We looked at his 'so-called' science in its general status and relations, and we found it to stand apart from all acknowledged sciences. We glanced over the contents of his exposition, and we found that his programme has not been carried out. We considered the logic of his system, and we found it no less ambiguous than the materials to which it is applied. We have now examined the result to which his systematic labours have attained, and we find him hopeless of a system. It remains to ask whether his failure is the failure of a particular author, or whether it is the failure of his psychological position 'as such.'<sup>49</sup>

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<sup>49</sup> The systems of Angell and Judd can be less certainly appraised, partly because they have so far been carried out only at the text-book level, and partly because their writers are more interested in function and genesis than in content. We may, however, note the following points.

For Angell, psychology is the science of consciousness, and consciousness is awareness (*Psychology*, 1908, 1, 222, 366, 442; cf. 5, 150, 185, 199, 228, 246ff., 337, 364f., 370, 373, 383ff., 399, 426ff., 430, 441ff., 444, 446). On its subject-side, consciousness is the observing activity of a self; 'we' are immediately aware of thoughts and feelings, of perceptions, images and emotions (2, 442f.; cf. 84, 302, 401, 408, 431, etc.). In its object-half, or on its content-side (content being taken in the broader sense), consciousness shows the two aspects of structure or content (in the narrower sense) and function (e.g., 201). States of consciousness are functionally unitary, but by logical abstraction may be divided into cognitive and affective (302, 436f.). [The position of attention is ambiguous. Attention appears to be a function of subject-consciousness, with a structure on the side of object-consciousness; there is apparently no discriminable attentive function within object consciousness: 80ff.] Both the cognitive and the affective functions are, in their own right, modes of awareness: the cognitive are informative (109, 170, 198, 201, 222, 248), the affective are evaluative awarenesses (302, 320, 322, 327, 378, 382). In general outline, therefore, Angell's system is very like Ladd's.

For Judd, likewise, psychology is the science of consciousness (*Psychology*, 1917, 1, 5, 10, 12, 38, 145, 309; behaviour is to be studied for the understanding and explanation of consciousness), and this consciousness is awareness (2ff., 6, 12f., 142, 238, 329; cf. 27, 65, 73, 160f., 166, 169, 183, 190, 212, 233, 246, 270, 276, 291, 345). Awareness, however, is always the awareness of 'someone'; it is 'one' or 'the observer' or 'the individual' or 'the child' that is conscious (1, 141f.,

### *The Functional Systems*

§ 4. We pass, accordingly, to a consideration of the features common to functional systems in general.<sup>50</sup> There seem to be four main tenets or tendencies which we may regard as characteristic of the school.

155, 169, 189, 241, 272, 301, etc.). 'Conscious processes,' which are classified according to nervous processes (64ff.), show the familiar duality. Over against sensations (73, 188), which are discussed content-wise, stand the attitudes of feeling and attention (66f., 146ff.), which are discussed functionwise. Perception includes, on the side of function, the activities of fusing, locating, distinguishing, recognizing, in a word of relating sensations (163, 166, 169, 186, 189, 191), and on the other side the product or resultant of these activities, the percept or relational fact, a content of a higher order (169, 175, 186f., 189, 191f.). The content of memory consists in images, which are primarily substitutes for sensations and percepts (241ff.), and its activity consists in recall according to the laws of association (244ff.). Ideation furnishes us with contents of a still higher order, difficult of precise description (246), and with such activities as conception, abstraction, generalization, judgment and reasoning (263ff.). Lastly, in voluntary choice we have a personality, an organized whole of mental activities (308), directed upon 'ideas' which are themselves organized "composites of experience" (306, 309).

There remains the question of the 'someone' who is thus in manifold ways and degrees aware. Judd's teaching is that we must conceive the knowing self, to which the study of conscious processes leads us, as an unitary being,—a 'conscious being' which, like the 'living being,' is an organized unity (274ff.; cf. 263). The self possesses and, by unifying, modifies conscious states (274). It is, however, difficult—since the terms 'self' and 'personality' are used interchangeably—to reconcile the statement that "the self is a being which perceives and forms concepts" (274) with the statement that "personality is the name of that individual nature which has been developed out of the play and interplay of impressions and instincts and conscious comparisons and imaginations" (308). See p. 521 above.

<sup>50</sup> This is not the place for a bibliography of functional psychology. I give a few early references, and name two papers of objective import.—W. James, "On Some Omissions of Introspective Psychology," *Mind*, ix., 1884, 18f.; *The Principles of Psychology*, i., 1890, 478; H. Ebbinghaus, *Grundzüge der Psychol.*, i., 1897, 161ff. (i., 1905, 176ff.; changed by Dürr, i., 1911, 175ff.); E. B. Titchener, "The Postulates of a Structural Psychology," *Philos. Rev.*, vii. 1898, 449ff.; "Structural and Functional Psychology," *ibid.*, viii., 1899, 290ff.; C. A. Ruckmich, "The Use of the Term 'Function' in English Text-books of Psychology," *Amer. Journ. Psych.*, xxiv., 1913, 99ff.; K. M. Dallenbach, "The History and Derivation of the Word 'Function' as a Systematic Term in Psychology," *ibid.*, xxvi., 1915, 437ff.

Angell remarks (*Psychol. Rev.*, xiv., 1907, 63) that 'structural psychology was the first to isolate itself.' In a broad sense, that statement is true; functional psychology claimed the whole field. In historical detail, however, things are less simple. There had already appeared in America the two overtly functional systems of J. M. Baldwin (*Handbook of Psychology*, 1889-91) and Ladd (1894). Moreover, it was as early as 1887 that Ladd, in his *Physiological Psychology*,



(1) The distinction between the 'activity' or 'function' and the 'content' or 'structure' of consciousness is recognised, explicitly or implicitly, by all the psychologists of function. It reflects, of course, the biological distinction of organic function and organic structure, of physiology and morphology. We say 'biological,' in deference to current usage: in point of fact, the distinction is rather technological than scientific. It belongs of right to the great technology of medicine; it was accepted, and found useful, by human physiology; it remained serviceable in the beginnings of comparative physiology. On the strict ground of science, it is no longer adequate to our knowledge;<sup>51</sup> and if it still persists in biological textbooks, the reason is again largely technological: the distinction recommends itself for pedagogical purposes. Even in medicine, its encouragement of a rigid specialisation lays it open to criticism.

There is, nevertheless, in the 'biological' context, a certain appeal to common sense, a certain plausibility, about this correlation; heart and lungs and hand and brain, regarded as machines, are obviously 'adapted' to the 'ends' which they subserve. We search in vain for any such plausible coördina-

sought deliberately to subsume the whole body of experimentally observed facts to a functional view of 'the nature of mind.' So that functional psychology had been a little uneasy, a little self-conscious, for a whole decade before the catch-word 'structural' came on the controversial scene. And the first note of revolt (E. W. Scripture's *New Psychology*, 1897) was struck in the physical, not in the biological key. Scripture's attempt proved abortive, partly because the physical concepts of time, space and energy were inadequate to the psychological data, and partly because he represented his new psychology as a straight development from the old: "there is no difference in its material, no change in its point of view" (453). 'Structural' psychology, as its name implies, recognized the existence of a correlative psychology of function; it isolated itself only to the degree that it demanded equal rights for content and activity, and by so doing protested against the autocracy of function. In the 'biological' atmosphere of its time the phrase did good controversial service. If the view of my book is accepted, both 'functional' and 'structural,' as qualifications of 'psychology,' are now obsolete terms.

<sup>51</sup> As appears, e.g., from such works as H. S. Jennings' *Behaviour of the Lower Organisms* (1906), and J. S. Haldane's *Organism and Environment as Illustrated by the Physiology of Breathing* (1917); cf. also B. F. Kingsbury, "The Fitness of Organisms from an Embryologist's Viewpoint," *Science*, N. S. xxxviii., 1913, 174ff.; "The Interstitial Cells of the Mammalian Ovary; *Felis domestica*," *Amer. Journ. Anat.*, xvi., 1914, 79; "The Development of the Human Pharynx," *ibid.*, xviii., 1915, 374ff. It is obvious that those who identify biological phenomena with physicochemical processes have no need of the concept of function (see e.g., J. Loeb, *Forced Movements, Tropisms and Animal Conduct*, 1918); but this view is not here under consideration.

tion of conscious content and conscious function. The "forms of receptivity," according to Ladd, stand in "bewildering and unclassifiable variety" over against the "relatively few forms of organising activity displayed in all mental states."<sup>52</sup> If content is to be made adequate to function, then (as in the instance of the 'representative image') function must be read into content, so that the content becomes an hypostatized function.<sup>53</sup> Nowhere is the attempt made to show that the strictly observable 'contents' are the suitable vehicles or instruments or substrates of the various modes of psychical activity.

Why, then,—it is natural to ask,—why, and with what hopes, did psychology submit itself at all to 'biological' guidance, and seek within its subject-matter for the analogues of organic structure and organic function? The answer refers us to historical conditions. Historically viewed, the functional psychology of which we have taken Ladd to be representative is a plea in avoidance offered before the court of science on behalf of empiricism. The continually growing body of experimental facts, menacing even while unorganised, had to be reckoned with; and the distinction of function and content afforded a means whereby it might be duly subordinated to the empirical system. If the worst should come, and experimental psychology should be able presently to organise itself as an independent science, still the empiricist had gained time, a breathing-space for adjustment, and had gained also a set of working concepts by whose aid the break might, so to say, be eased and graded. What he expected, however, as is clear from all of Ladd's work, was a new lease of life for the traditional empiricism. The particular line which his reconstruction followed was settled for him by the status of science at the time. Not mathematics, not physics, was the characteristic modern 'science,' but biology; and if biology thought and spoke in terms of structure and function, psychology need not scruple to think like thoughts and to use like language. So there arose a new empirical psychology, in all essentials at one with the old, but more or less effectively disguised under the cloak of contemporary science. After the event, we can see that psychology borrowed in haste, without assurance that the loan could be turned to properly

<sup>52</sup> *Psychology*, 53ff., esp. 57.

<sup>53</sup> *Ibid.*, 234ff., 244ff., 376. D. S. Miller ("The Confusion of Function and Content in Mental Analysis," *Psych. Rev.*, ii., 1895, 536) meets the difficulty by a reference to the unlikeness of physical cause and physical effect. But content does not stand to function as cause to effect, and physical causes and effects are alike energetic.



psychological account. We can see, further, that the biology to which psychology appealed was not in truth a scientific biology, but a biology conventionalised and popularised. At the time, nevertheless, it was only natural that the contents and functions of this new empirical psychology should seem to bear the authentic stamp of science.

(2) In the second place, functional systems are disposed to correlate the phenomenon of consciousness with unreadiness or inadequacy of the nervous system; "consciousness is only intense," remarks James, "when nerve-processes are hesitant."<sup>54</sup> As soon as the organism is confronted by a problem which its existing neural organisation is unable to meet, consciousness steps in to dispel the perplexity; "straightway appears consciousness, with its accompanying cortical activities, taking note of the nature of the stimulus and of the various kinds of muscular response which it called forth."<sup>55</sup> The point must be taken, it would seem, with a certain reservation: for it is not unreadiness or inadequacy in general that is responsible for the appearance of consciousness, but rather such unreadiness and such inadequacy as require, within the biological limitations of the particular organism, only a certain additional prompting or urging to change into readiness and adequacy. With this reservation more or less expressly made, some authors, like Angell, declare in round terms that "conscious activities emerge at the point where reflex acts are found inadequate to meet the needs of particular situations;" "if the reflexes and the automatic acts were wholly competent to steer the organism throughout its course, there is no reason to suppose that consciousness would ever put in an appearance."<sup>56</sup> Others, like Dewey, argue that difficulties in

<sup>54</sup> *Principles*, i., 1890, 142. James, although he favoured a functional as opposed to a structural psychology ("The Energies of Men," *Philos. Rev.*, xvi., 1907, 2), does not give us in the *Principles* a system of functional psychology. He gives us a work on the principles of knowledge, written from a psychologistic standpoint. If his volumes are read with this interpretation in mind, the critics' charge of lack of plan (which James repelled: *Text-book*, 1892, iii. f.) will be found groundless.

<sup>55</sup> Angell, *Psychology*, 1918, 64.

<sup>56</sup> *Loc. cit.* Angell confines his discussion, in these passages, to the human consciousness. The question 'why' the human infant should be limited to a certain group of inherited coordinations he hands over to 'the biologist' for answer. So one might hand over many other questions: 'Why,' since in the last resort the environment is for all organisms the same, should any infant embark on a career for which his inherited coordinations are insufficient? and 'why,' having thus embarked, should he again meet with limitations to his conscious powers? and so on. The biologist, in so far as he is man of science, will maintain a discreet silence.

the way of nervous conduction heighten the clearness of consciousness, but do not commit themselves outright to a theory of its first appearance.<sup>87</sup> There are thus differences of detail.<sup>88</sup> It would, however, be widely agreed that, at any rate in the case of man, the 'condition' of consciousness is a temporary and corrigible imperfection of nervous organisation.<sup>89</sup>

A view of this sort seems, indeed, to be logically bound up with the view that consciousness is primarily and actively a matter of function, and only secondarily and passively a matter of content. For if consciousness is to do us some organic service, it will find its natural opportunity in some defect of our given (non-conscious or extra-conscious) organic equipment; and this, in the categories of our current thinking, means some defect in the functional capacity of the brain. Positive evidence, however, is found in the experience of every adult. We have learned to write, to use a typewriter, to ride a bicycle, to play a piano, to drive a motor car; and we know that, as the original problems cease to baffle us, and the skilled movements become habitual, the need of conscious 'control' grows less and less; we find that consciousness 'intervenes' less frequently and less imperatively, until it may finally disappear. Could there be more convincing proof of the instrumental function of consciousness? and is not the

<sup>87</sup> So at least I understand these discussions. See J. Dewey, "The Theory of Emotion," *Psychol. Rev.*, i., 1894, 553ff.; ii., 1895, 13ff.; "The Reflex Arc Concept in Psychology," *ibid.*, iii., 1896, 357ff.; "The Psychology of Effort," *Philos. Rev.*, vi., 1897, 43ff.

<sup>88</sup> The same general idea has been expressed in various places by W. McDougall. See, e.g., "A Contribution towards an Improvement in Psychological Method," *Mind*, N. S. vii., 1898, 159ff.; "On the Seat of the Psycho-physical Processes," *Brain*, xxiv., 1901, 607f.; "The Physiological Factors of the Attention-Process," *Mind*, N. S. xi., 1902, 341f.; *Physiological Psychology*, 1905, 59f.

It is possible that Ladd is adopting a like view when he makes conation or conscious striving "the most fundamental of all psychic phenomena" (*Psychol.*, 219). Ladd and Woodworth, however, combat the theories of Dewey and McDougall (*Physiol. Psychol.*, 1911, 610ff.).

<sup>89</sup> Judd ("Evolution and Consciousness," *Psychol. Rev.*, xvii., 1910, 77ff.) forms a marked exception to the rule. Consciousness appears only when the organism has attained a certain stage of complexity of inner organization (94). The question of first causes—where this consciousness comes from—is not raised (92). Having appeared, however, consciousness functions as a centre of reorganization (93f.) The more highly organized, *i.e.*, the more self-sufficing the organism, the greater is the rôle and the higher the development of consciousness (80f., 88). In a word, consciousness is at once the product, the index, and the cause of organization.

formation of a habit typical of the general course of the mental life?<sup>60</sup>

We must here meet the psychologist of function on his own ground; we must, that is to say, grant him his assumption that consciousness, the subject-matter of psychology, is awareness. The question then arises whether our conscious life is, as a matter of fact, nothing more than a recurrent grappling with problems. Are we aware only when and in so far as we are searching, reconciling, reducing to familiar terms, trying to understand,—or whatever other words there may be that imply the setting of a problem and the urgency of its solution? Surely we are not; surely there is a contemplative as well as an acquisitive awareness. We are not always learning something new, or practising this new until it becomes old. There are times when we sun ourselves, as it were, in the full light of consciousness,—times when, without any “felt necessity for further nicety of adaptation to surroundings,”<sup>61</sup> we are none the less keenly and competently aware. There is a consciousness militant, but there is also a consciousness triumphant: more than that, there is a consciousness that, in the midcourse of its campaigning, rests quietly upon its arms, and surveys the terrain it has occupied. A narrowly instrumental theory of consciousness comes very near to a *reductio ad absurdum* of the biological ‘struggle for existence.’

(3) The two characters that we have so far discussed—the distinction within consciousness of content and activity and the assignment to consciousness, especially in its active phase, of a value for organic survival—lie on the surface of the functional systems. Underneath, always and everywhere, runs the steady current of teleology. It is one of the ironies of the history of science that the great biological generalisation which was to free us of teleology in our study of the phenomena of life should give rise, in post-Darwinian days, to an unbridled license of teleological ‘interpretation.’<sup>62</sup> Biology

<sup>60</sup> Judd is here consistent. “Too often the psychology of habit has been guilty of the statement that habituation leads to unconsciousness. This is not the case” (*Psychol.*, 1917, 207).

<sup>61</sup> Angell, *Psychol.*, 1908, 74. Note the implication of the phrase! Consciousness, which might on Angell's terms be described, roughly, as the “felt process of adaptation,” here anticipates its own function.

<sup>62</sup> “If we apprehend the spirit of the ‘Origin of Species’ rightly, then, nothing can be more entirely and absolutely opposed to Teleology, as it is commonly understood, than the Darwinian Theory:” T. H. Huxley, “Criticism on ‘The Origin of Species’” (1864), *Lay Sermons, Addresses and Reviews*, 1887, 264. It is a mind “stored with the choicest materials of the teleologist that rejects teleology,”—so J. Tyndall speaks of Darwin (*Report B. A. A. S.*, 1874, 1875, lxxxvi.). Cf. E. du Bois-Reymond, *La Mettrie*, 1875, 23.



has suffered, and is still suffering, from that license. Biology, nevertheless, again furnished psychology with the obvious scientific parallel. Biologists spoke in terms of final cause, the whole system of biology was pervaded by teleology, and psychology again had no call to be more scientific than science. We have seen the outcome in Ladd's thinking: a teleological activity of organisation plays upon a causally (or quasi-causally) determined content, to the inevitable confusion of the concept of consciousness which is fundamental to his psychology at large. We find a like ambiguity in Angell's book. Mind, as we have said, there "seems to involve the master devices" through which the "adaptive operations of organic life may be made most perfect," and "the real business of consciousness is to be sought amid the adaptive responses of the organism to its life conditions."<sup>63</sup> But when we go in search of master-devices and real business we are told that the basal distinction is that "between certain kinds of nervous activity overtly involving consciousness . . . and certain other kinds not overtly involving it;" consciousness is only the "index" of "problem-solving adaptive acts." Nay more: if we speak "as though mind might in a wholly unique manner step in and bring about changes in the activity of the nervous system," we are employing "convenient metaphors," "a convenient abbreviation of expression," which must not blind us to "the fundamental facts which lie behind."<sup>64</sup> Such are the logical sacrifices that teleology demands of her children!

May it not be, however, that the fault lies with the children? May it not be that the psychologist of function takes his teleology a little carelessly? It has come down to him from the older empiricism; it is guaranteed by philosophy and technology; it is justified by biological example. Small wonder, then, that he should slip easily, even heedlessly, into the teleological attitude! But are we on that account ruthlessly to banish teleology from a scientific psychology?

One could wish, certainly, that the functional systems were somewhat less confident of their position. For it is one thing to affirm broadly that "the stream of consciousness appears . . . as a current designed from the beginning . . . to the fit performance of a certain work,"<sup>65</sup> and quite another thing to build psychological facts and laws, the details of psychology, into a coherent system of means and ends. The

<sup>63</sup> *Op. cit.*, 8, 95.

<sup>64</sup> *Ibid.*, 59f., 300.

<sup>65</sup> Ladd, *Psychol.*, 668f.

teleology which is used in psychology as a tool of scientific construction is, as a matter of fact, both clumsy and double-edged. The psychologist may insist on final causes as he will, but he never makes them adequate to the refinement of observation;<sup>66</sup> and he may answer any number of Whys, but he is still faced by unanswerable Why-nots that throw doubt upon his positive explanations.<sup>67</sup> Yet we must insist that final causes, if they are at all recognised in psychology, be recognised primarily for psychology's sake. It is not enough to infuse just so much of teleology into the psychological system as shall orientate us toward ethics or history, or place us within a generally biological context, or blur the difference between 'pure' and 'applied' science. Psychology has its own claims in behalf of every item of its subject-matter.<sup>68</sup>

There is, however, no reason to suppose that these claims could be met by any teleology, even the most self-conscious and the most persistent. The whole history of science argues to the contrary. We may freely grant that teleological ideas have the occasional heuristic value which certain investigators

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<sup>66</sup> "Fortunately for science," writes Judd (*Psychol.*, 90f.), "there have been a few cases in which the same person has been able to observe directly both the normal color sensations and the partially color-blind series." Fortunately for descriptive science, truly: but how fortunate for psychology in Judd's sense? How has the development of red-green vision aided man in the struggle for existence(4)? or what has man gained by the "unique compromise process"(92) which gives rise to the purple sensation? These and like questions are not touched.

<sup>67</sup> "It is because we have . . . no special organ affected by weak currents of electricity that men overlooked for so long a period both the prevalence of forms of electrical energy and the close relation between light and electricity" (Judd, *op. cit.*, 72). But, granted that the facts are as stated and granted that this furtherance of knowledge is useful, why have we not the special organ?—for it is surely evident that biological conditions, which have produced the 'electric fishes,' are also competent to produce an electrical sense-organ in man.

Again: "We do not have microscopic eyes like the fly. Nor . . . do we have distance vision like the eagle's. . . . The range of human vision has been determined by the range of possible human reactions" (132); cf. W. H. Hudson, *Idle Days in Patagonia*, 1893, 183 f. But has not man extended his reactions by microscope and telescope, and are not tools (249) sensory as well as motor? Cf. H. Spencer, *Principles of Psychol.*, i., 1881, § 164, 365; O. Wiener, *Die Erweiterung der Sinne*, 1900.

<sup>68</sup> How utterly this subject-matter may drop out of sight, under the prepossession of teleology, appears in the discussion by A. E. Taylor, *Elements of Metaphysics*, 1903, 306f.



have claimed for them.<sup>69</sup> It remains true, notwithstanding, that these same investigators have only by exception had recourse to teleology, and have not been encouraged to adopt it as a guiding principle of research. It remains true that final causes have long been banished from the domain of the older sciences, and that they flourish only where (and in proportion as) exact knowledge is wanting. It remains true that interpretation by means and end tends to close enquiry and thus to bar the progress of scientific knowledge. In particular, it is highly significant that biology, after full trial of teleological principles, is in these latter days resolutely turning away from final causes to the laborious planning of experiments and the patient accumulation of observed facts,—so that ‘evolution’ and ‘heredity’ and ‘adaptation,’ once the means whereby we conjured ends, are now coming to be mere descriptive labels for laboratory note-books. The whole history of science thus goes to show that teleology is essentially non-scientific. And, if that is the case, there can be no room for “teleological import” within a “science of the life of the mind.”<sup>70</sup>

(4) Finally, this teleological attitude threatens the stability of psychology as an independent branch of knowledge. The psychology of the functional systems appears as transitional, as a stage either upon the difficult ascent toward philosophy or upon the level road that leads to various application; always it appears as a half-way house on the journey to something else, and not as an abiding-place. The individual psychologist may look forward to the one goal or to the other, or may perhaps keep both in view; that is a matter of temperament and training. It is at any rate characteristic of the school that they are not content to rest in psychology. Their psychologising, with whatever pains and seriousness it is done, seeks to transcend itself, as if in the last resort it were done not for its own sake but for the attainment of some foreign end.

Ladd, for instance, bears witness on the side of philosophy. “The problems of philosophy,” he tells us, “all emerge and force themselves upon the mind in the attempt thoroughly to comprehend and satisfactorily to solve the problems of a scientific psychology; and the attempts, along the different main lines of research in psychology, to deal scientifically with

<sup>69</sup> E. Mach, *Die Analyse der Empfindungen*, etc., 1900, 60f. The instance cited is not physical but biological. I do not find in the *Erkenntnis und Irrtum* (1906) any acknowledgment of the heuristic value of teleology in physics, though the book as a whole contains a good deal of teleological thinking.

<sup>70</sup> Ladd, *Psychol.*, 668.

its problems all lead up to the place where this science hands these same problems over to philosophy."<sup>71</sup> Psychology is therefore of value as a propaedeutic to philosophy, in so far as philosophy is too difficult and too intricate to be approached directly and without an introductory easement; but the psychologist cannot, in scientific status, compare with the physicist or chemist or biologist. On the contrary! The longer he works in psychology, the more clearly does he confess his inability to tackle his problems at first hand; and the more able he is, the briefer will be his psychological apprenticeship to philosophy.

Judd, on the other hand, lays stress on the importance of psychology for the understanding and consequent control of human life and human institutions. He devotes a chapter of his general *Psychology* to the maxims of 'mental hygiene,' by following which the individual may attain to the highest level of organisation, "when mental development becomes a matter of voluntary control." He devotes another chapter to the institutional applications of psychology, its usefulness in literature and the fine arts, in the social sciences and anthropology, and especially in education. He recognises, to be sure, that psychology's relation to philosophy is "closer than that of any of the special sciences," but the recognition is formal and carefully qualified. The main interest of the study of consciousness is that it gives the key to human nature, and thus enables us intelligently to guide the course of the individual and of society.<sup>72</sup>

There is no need of further examples, but there is great need that the reader clearly understand what these particular examples are meant to show. The point here at issue is not that the author of a scientific text-book should shut himself up within the four walls of his specialty, and look neither above nor about. Where we find such narrowness of vision, we are likely to find also confusion of thought. The point is rather that we note a marked difference of emphasis between text-books of functional psychology and text-books of other sciences. A text-book of physics will discuss various types of engines and machines, but it will discuss them as illustrative of physical laws. A text-book of physiology may discuss various types of pathological phenomena, but it will discuss them in connection with physiological methods and

<sup>71</sup> *Philos. of Mind*, 1895, 73; *Psychol.*, 12. Cf. J. R. Angell, "The Relations of Structural and Functional Psychology to Philosophy," in *The Decennial Publications of the University of Chicago*, iii., 1903, 55ff.; *Psychol.*, 1908, 9ff.

<sup>72</sup> *Psychol.*, 1917, 314ff., esp. 324, 344ff.; cf. 10, 208, 268, 299f., 309.

physiological principles. The text-books of functional psychology, on the other hand, tend—it is true, in varying degree—to make of psychology either an introduction to philosophy or an aid to individual and social welfare. In so far as these tendencies prevail: in so far as functional psychology, in its exposition of psychology proper, goes out to meet the problems of philosophy or of our customary human life: in so far there is real danger that the pains and seriousness which are the due of psychology as science are withdrawn from psychology and expended in those other fields.—

These, then, seem to be the four main characteristics of the functional systems. The subject-matter of psychology is duplicated, though function is preferred to content; consciousness is a solver of problems; the whole course of the mental life is regarded teleologically; and psychology is written as a preface to philosophy or to some practical discipline. We have already indicated that these characters are not logically coördinate. A logical arrangement might, however, have appeared to prejudge the case, whereas our topical presentation has required that every character be discussed on its individual merits. In any case it should now be plain that functional psychology has its roots in the Aristotelian empiricism, and that while it has taken color of modernity from the surrounding sciences it has not adopted the modern conception of science itself. For this reason Ladd's deliberate and sustained effort to maintain the continuity of psychology as science was foredoomed to failure. His loyalty to the past is incompatible with his open-mindedness toward the future. He recognises, frankly if a little anxiously, the gradual emergence of the scientific problem, yet he cannot bring himself to discard, even in what he defines as science, the constructions of prescientific thinking. He is thus betrayed into an illogicality which, in any other context, he would have been among the first to discern.

## CHURCH HISTORY AND PSYCHOLOGY OF RELIGION

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The following paper is written by a student of church history, not by a psychologist. It attempts to emphasize, by citing certain specific examples, the importance and necessity of testing historical conclusions by the norm of psychological law. Furthermore, it ventures to criticise the present work of religious psychologists from the historian's point of view, and to suggest certain lines of investigation and verification in which he seems most to need professional assistance.

### I

Formal instruction in religious psychology is given in most of our better theological seminaries; and more than this, it has become a commonplace in clerical circles that theologians ought to study psychology. There is thus an enormous amount of desultory reading of psychological treatises and text-books in ministerial studies. Unfortunately, however, a greater part of this study goes no further than a genetic investigation of the religious consciousness in a normal mind. It is Starbuck and his disciples whose writings are read rather than James, of whom indeed I believe it must be said that he writes altogether above the heads of many clerical readers. As a result, there is little reaction upon historical conceptions from the newer scientific knowledge of the human mind; only perhaps in his notions of the Crusades, or of the great religious revivals, is the student of ecclesiastical history directly influenced by his psychological readings. In these two matters his study of the mob-mind in such works as the *Social Psychology* of E. A. Ross inevitably becomes an element in the formation of his historical judgments.

But it is neither the religious experience of normal man nor great popular enthusiasms that most concern the student of ecclesiastical history; usually, the most difficult problem in his investigation of any period must be the understanding of the mind of a religious genius whose dominance over his fellows forms the figures which are printed upon the canvas recording the religious experience of the age. The reactions of normal men and women upon the teachings and examples

of a leader constitute the warp and woof of the fabric, but this must usually remain unwritten and unwritable history; certainly it can never be reconstructed, be it never so tentatively, where one's understanding of the leader is distorted or untrue,—as it is quite certain to be when sympathetic insight into the mental processes of the religious genius is totally wanting.

Much time and labor are spent by modern students of the formative centuries of ecclesiastical history in tracing out, by philological evidence alone, influences and origins of constituent ideas and practices in various systems. Indeed, to some writers the ideal history of the course of religious thought seems to be a genealogical tree of theological "schools." Almost every German scholar who has attempted a *Dogmengeschichte* during the past seventy-five years is obsessed by this idea. Or perhaps a better illustration would be to select from the enormous literature upon Christian Gnosticism such work as is represented by the volumes of Bousset,<sup>1</sup> Anz<sup>2</sup> and Amélineau,<sup>3</sup> who demonstrate respectively that the system sprang from Zoroastrianism, Babylonian astral worship and Coptic magic, each arguing exclusively from the philological antecedent of certain specific terms used by Gnostic writers. All of these scholars know and mention explicitly the religious syncretism which marked the countries and age in which Gnosticism arose,—that strange coexistence and intermingling of all sorts of exotic cults which people frequented in seeming indifference to their inconsistencies and contradictions. Yet, in spite of the established fact of this religious climate, none of the three authors I cite has hesitated to ascribe definite lines of direct derivation where the evidence justifies no further conclusion than the contemporaneous existence of these other religions. With them it is not, as it should be, a commonplace in their historical canon that the religious genius habitually seizes out of the air, as it were, a name or phrase or idea that is current in his day, and weaves it into his own system without the slightest consideration of its origin and proper significance, and often indeed without any further acquaintance with the system to which it originally belonged.

Where the evidence is scanty enough, the fallacy of spinning an elaborate theory of genetic relationship out of philological coincidences is not ridiculously evident; but an attempt to apply exactly the same method to isolated identities, where

<sup>1</sup> W. Bousset, *Hauptprobleme der Gnosis*, 1911.

<sup>2</sup> W. Anz, *Zur Frage nach dem Ursprung des Gnostizismus*, 1897.

<sup>3</sup> E. Amélineau, *Essai sur le gnosticisme égyptien*, 1887.



the general evidence is more complete, will usually lead to all sorts of obvious inconsistencies. For example, in the *Book of Mormon* there are phrases which imply the technical terminology used in a contemporary controversy over baptismal remission which divided the denomination known as Disciples.<sup>4</sup> If there were extant only the *Book of Mormon* itself and a contemporary anti-Christian polemic writing which contained, as quotations, scattered fragments from sermons by Disciple ministers, a future Bousset or Anz, if he were true to his method, would have to describe Mormonism as a subcult or offshoot of the "Disciple School."

For another example of failure to consider in historical deductions obvious mental peculiarities of the religious genius, one may look into the literature which has sprung up around the interpretation of the fact that Jesus spoke of himself as the "Son of Man." All sorts of far-fetched theories are drawn from this phrase in sermonic literature; but the opinions of scientific historians are scarcely less numerous and divergent. Aramaic and Syriac lexicography has been ransacked, arrays of texts are marshalled, and statistical tables compiled therefrom; but all on the tacit assumption that the term in question was deliberately chosen in the first place to represent a clearly defined and fully developed idea, and that it was used in exactly that sense and no other up to the end. So firmly established is this assumption that none of the writers upon the subject, so far as I know them, has given more than passing reference to Ezekiel's use of this term. As he used it, the phrase is simply a circumlocution for "a prophet;" Jesus, during the period reported in the Gospels, undoubtedly meant by the "Son of Man" one greater than the prophets; hence Ezekiel is ignored in favor of Daniel, who uses the term with messianic significance in words quoted by Jesus himself. Yet it is an outstanding fact in the biography of almost any religious genius that his terminology grows with his religious experience. Words that were used early in his career with simple literal significance are gradually filled with new meaning, until they become so highly technical that only a mind saturated with the whole doctrine may fully appreciate them. In the same way it is not a rare phenomenon to find a religious innovator whose system is directly influenced by the choice of his terminology. If in the teaching of other men he discovers connotations and definitions of certain words that he himself has used almost by

<sup>4</sup> Wm. A. Linn, *The Story of the Mormons*, 1902, 93 (footnote).

chance, he may unhesitatingly graft these new ideas into his own system.

If in the light of these psychological peculiarities we suppose that Jesus, early in his career, still looking upon himself as one of the religious leaders known as prophets, adopted Ezekiel's habit of designating himself as the "Son of Man;" and then, in the development of his messianic consciousness, brooding over the Daniel prediction, gradually added to the simple significance the more complex and technical connotation; we shall have a theory which will not only meet the facts of the case but which has the additional strength of support by psychological parallels in the history of other religious founders.

But nowhere perhaps in historical investigation is there greater need of a correct canon of psychological interpretation than in those problems which are concerned with pseudographic writings. To speak of these as forgeries and plagiarisms is to introduce ethical considerations which any study of the abnormalities of religious genius will show are entirely lacking. A greater part of Biblical criticism involves questions concerning documents of just this kind; and it would sometimes seem that the most formidable obstacle in the path towards a scientific solution of the problem is that complex of prejudices and repugnances which arises in most minds the moment it is suggested that a particular document is not from the hand of the author which it claims. The naïve procedure of a pupil who cuts out of his Bible every word and passage that any writer has declared "not authentic" is scarcely less scientific than the refusal of 'conservative' writers on Biblical criticism and patrology to consider evidence and arguments that they would accept unhesitatingly were the documents in question of secular instead of ecclesiastical origin.

There is, for example, the problem of the *Second Epistle to the Thessalonians*, which is still accepted by many modern scholars as of Pauline authorship, though perhaps the majority have come to deny its authenticity, on the grounds of its formal verbal identities with the *First Epistle*, and its explicit denial of doctrine propounded in that document. The general tone of the *First Epistle* is benignant, enthusiastic and impetuous; and it speaks of the second coming of the Lord as imminent, during the lifetime of the writer and those he addresses. The general tone of the *Second Epistle* is chiding, repressive and authoritative; and it teaches that before the second coming of the Lord certain events must occur, and presumably that

ages will first elapse. Furthermore, it denounces roundly those who refuse to perform their routine duties in life. Besides the fact that most of the introduction follows the *First Epistle* word for word, there is the significant statement that the letter is written lest "ye . . . be troubled, either by spirit, or by word, or by *epistle as from us*, as that the day of the Lord is just at hand . . ."

To reconstruct in the following terms the circumstances under which this second document was written, is not, it would seem, a gratuitous flight of the imagination, but as scientific a procedure by the laws of psychological analogies as it is for a palaeontologist to sketch an extinct animal from the evidence of some fossil bones. Certain early Christians were so impressed with the imminence of the Second Advent that they dropped the distractions of routine life and gave themselves entirely to contemplation and preparation for the catastrophe. In refutation of their fellows who disputed this theory, they exhibited as the words of Paul what we now have as the *First Epistle*. Among the anti-adventists who refused to be silenced by this argument was one who offered in refutation, not a rival interpretation of the passage, but a whole-hearted denial that Paul wrote the words at all; some evil-minded person had forged the whole epistle or had tampered with the text. Next, brooding over the matter, he undertook to rewrite the epistle as Paul probably had written it, copying exactly the insignificant introduction, but changing entirely the doctrinal portion, omitting the personal references that had only temporary significance, but warning explicitly against forgery as if it were foreseen, and railing at those who refused to work. With the revision once written, the next easy and final step would be the gradual conviction in the mind of the revisor that the document thus produced was wrought under divine inspiration or by the miraculous spiritual dictation of Paul himself. Later generations, ignorant of the local controversy of their fathers, would, of course, treasure both documents without distinction since each professed to be from the hand of the revered apostle.

## II

These three examples of the ecclesiastical historian's direct application of methods and materials that are purely psychological are given, not so much for their intrinsic value, as in the hope that such a concrete presentation of sample problems may start some psychological reader upon investigations consciously directed towards the solution of other historical



questions. For there are numerous matters in which generalizations derived casually from readings in religious biography cannot meet the needs of the historian; in these he must await the scientific pronouncements of the professional psychologist. It may not, therefore, be amiss if, before sketching the main outlines of a few of these open questions, I attempt to describe the kind of solution that will best meet the needs of historical investigation.

In the first place, it must always be borne in mind that history is concerned with immediate, not ultimate psychical causation. However important the primitive origin of any given psychical manifestation may be in the eyes of pure science, it is only those phases which emerge from the mental surface that belong to the field of history. Psychoanalysis may be able to trace the religious ideas of an individual behind the veil of his consciousness, but the historian has no right or authority to penetrate that dim realm. Roughly speaking, it may be said that the historical record of any religious figure is complete when it has accurately described what he believed that he believed, how he presented his belief, and the way in which his followers understood that presentation; in the main, the course of religious history consists of little more than an overlapping series of these triple elements.

It is extremely unfortunate, so far as the study of history is concerned, that modern psychology is so largely analytical and so sparingly descriptive. In a science so young it is natural, of course, that the general problems should be worked and reworked many times before specific manifestations can be brought to the front; but the religious historian awaits somewhat impatiently the growth of an adequate mass of studies of specific religious types. Though it be not altogether just, he is inclined to complain that in general, when psychologists do treat of religious phenomena, they use such matters only to illustrate some broader psychological hypothesis, or that frequently, being interested in the main thesis, their selection of fact is not a happy one.

Similarly, in the eyes of the historian, it is unfortunate that in discussing religious experiences the psychologist so frequently uses imperfect written records rather than first-hand, modern sources. Looking at the field in a broad way, one can not but be struck by the fact that there is a situation which may be caricatured in these terms: one writer describes the psychology of Joanna Southcott by analogy to that of Joseph Smith, another justifies his estimate of Joseph Smith by the presence of similar mental peculiarities in Joanna Southcott.

But it is inevitable that such cross-arguments must continue until we have adequate scientific studies by competent psychologists upon contemporary religious founders. The religious genius is always with us; probably, if one were to count them over, he would find no decade since 1800 which has not seen the establishment in America of a new religion which won more converts during the first twenty-five years of its existence than did Christianity in its first two centuries. No generation lacks its believers in a gospel written yesterday, or even in a contemporary incarnation of divinity. The historian cannot express too emphatically his hope that the religious psychologist will leave to him the biographies of Joanna Southcott, and will go out with a note-book to some dingy hall where a new gospel is preached. Nor can he repress a sigh of regret when he notices that James entered on his career in the Philosophical Department of Harvard one year before Mrs. Eddy opened her "Massachusetts Metaphysical College" in Boston, and then remembers how many of the "varieties" James chose to describe were drawn from the pages of eighteenth century Quaker biographies. Indeed, the same historian must not be judged too harshly if he sometimes declares that James' work, as a psychological treatise, is inferior to Howells' "Leatherstocking," despite the admixture of fiction in the latter.

### III

The problems themselves that demand solution are obvious. Division of the individuals to be described into three classes according to their respective functions in the course of a religious movement will show (*a*) the religious genius, (*b*) his personal disciples, and (*c*) ordinary adherents to the system. The groups produced by this functional classification seem to show distinct types of temperament, which deserve separate study.

Of the first of these little more need here be said. The generalizations that the historian has made concerning the psychological abnormalities of the religious genius need verification and restatement; and undoubtedly there remain more traits and habits of mind common to this type which will be discovered only by careful comparative study of living subjects. It has already been pointed out that material for such first-hand investigation is usually available.

In the examples cited in the first part of this paper we have not mentioned the second group, the personal disciples of a founder. These disciples seem to have played an important



part in the development of every religious movement. Though they lack the leader's originality, their influence may have been more or less decisive both by their polarization of his teachings as it passed through them to others, and by the reflex action of their understanding of his message upon the master's own final formulation of it. Usually historical sources preserve but little record of these disciples beyond their names, yet now and again one catches hints of the characteristics common to this type,—abnormal willingness to believe, fiery enthusiasm, unbounded liberality to "The Cause," and a lack of candor, often approaching deliberate trickiness, but without that common sense in practical values that sometimes stands out so amazingly in the genius himself.

In the third class the characteristics of the type will usually be found in the highest development in the official clergy; but any study of these professional religionists must be corrected by a corresponding examination of representative laymen, that due allowance may be made for the elements that belong, not to the religious experience, but to the office. That the different religions present specific types and sub-types must be the conviction of every curious observer; whether or not these types are persistent and amenable to classification is another question. Certainly the value of such classification, should it prove practicable, would be so great that one may hesitate to pass over as valueless even so frivolous an observation as Mozley's remark concerning "evangelical shallowness;" "How is it that goodness, poverty, and a certain amount of literary and religious ambition produce an unpleasant effect on the skin?"<sup>5</sup>

From the historian's side these characteristics, whether they be cause or effect, are of supreme importance; if certain temperaments are attracted by corresponding climates in religion there will follow, in the course of years, a reflex action upon the religion itself, tending toward more exact conformity with the religious taste of that temperament. If the existence of such a reaction can be definitely established, it will do much towards explaining on rational grounds the frequent recurrence in religious history of particular phenomenal series. Some writers are inclined to explain these on genealogical lines, bridging any chronological gap with a bold hypothesis of direct influence, while others, with less success, seek to explain them by laws drawn from a philosophy of history devised to meet

<sup>5</sup> T. Mozley, *Reminiscences chiefly of Oriel College and the Oxford Movement*, 2nd ed., i., 1882, 243.

the occasion; still other writers are content to remark that religious history persistently "repeats itself."

In the same way it is probable that the development of descriptive religious psychology will produce much material of importance in the solution of analytical problems. For example, those disquieting figures that appear in every religious body, who admit in private without scruple that they disbelieve in the possibility of any religion at all, and in public are the most zealous observers and advocates of their official creed down to its smallest denominational peculiarities, will probably be found to represent a distinct type of religious experience. Because these men appear most frequently in ritualistic circles, it is often said of them that "their religion is purely aesthetic." If this phrase, which sounds like an explanation, be true, it will be a fact of enormous significance in any attempt to define the religious sense. If on the other hand, as seems more probable, the religious experience of such men is really religious despite the denials of their materialistic metaphysics, that likewise will be a fact of far reaching implications.

Finally, it will be noticed that not until there has been accumulated an enormous bulk of descriptive investigations will it be possible to define the religious experience of normal human nature. Religion necessarily offers many attractions to the abnormal human mind; the high proportion of freaks in any group of religious has persuaded not only many laymen but some scientific men as well that all religious experience is fundamentally pathogenic. Whether this be true or not is, however, a matter of no great significance either for the historian or the psychologist; to both "religion is an incident in human history and a manifestation of human nature," and as such will remain an object for scientific study.

## DEATH-PSYCHOLOGY OF HISTORICAL PERSON- AGES

By ARTHUR MACDONALD, Washington, D. C.

A summary of the last words of those distinguished people in history, records of whom have come down to us, is a psychology of their death.

Before presenting the results of such a summary, and in order to understand better the significance of words during the dying hour, it may be useful to note a few points as to death itself.

### THE DYING HOUR

In another place, the author has treated in detail the physiology and psychology of death.<sup>1</sup> Here it may be stated that death is neither rapid nor sudden, but is preceded by a period of transition, which begins as soon as the reactionary forces of the organism have ceased and combat has ended.

The death-act is often confounded with the symptoms of disease which precede it. Dying begins after these symptoms have subsided; there is a pause in nature, the disease has conquered, the battle is over, and all is tranquil.

This transition-stage, or dying hour, may last for a longer or shorter time; in the great majority of cases persons are unconscious. Thus the natural death appears to be a brain-death.

But when there is consciousness during the dying hour, it depends upon nutrition and provision of the brain with blood. As there are three ways of physical death, (1) by brain, (2) by heart, and (3) by lungs, so there are three kinds of psychological death. The first is where there is little or no delirium, and intelligence not only continues to the end, but becomes very acute; physical prostration appears to be replaced by intellectual exaltation. Another kind of psychological death results from diseases only secondarily connected with the brain; the mind is in a mixed state between reason and delirium. The third kind of mental death includes all the lesions of the brain, which are almost always accompanied by loss of understanding; delirium is a symptom; there is a general obscuration of intelligence, and complete loss of consciousness.

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<sup>1</sup> *Medical Times*, N. Y. City, 1921.

## FEAR OF DEATH

In life the fear of anything is often much worse than the thing itself. This is especially true in the case of death. When the dying hour comes, the fear of death disappears.

Whether it is the brain, heart or lungs which give the signal of death, the brain-forces are usually weakened or destroyed first, causing sensation to lessen or cease. Whether there be consciousness to the last, or only at times, depends upon the nature of the disease and the mental and moral character of the person dying; and this in connection with surrounding conditions. In old age, death is the last sleep, showing no difference from normal sleep. The general consensus of opinion based upon the experience of all ages is that the dreadfulness of death and its physical pain are for the most part in the imagination.

## PSYCHOLOGICAL SUMMARY OF DEATH OF DISTINGUISHED PERSONS IN HISTORY

The average man usually dies unknown; whatever he thinks and says is soon forgotten. Fine and significant words may be lost. Now and then a physician may take the pains to note the last words of some of his patients. There may be a dozen cases, or more; but the outcome is small compared with the great number of those dying every day. Therefore almost all the last words recorded at death are those of eminent and distinguished persons.

This table represents a first attempt to summarize the mental condition, at or just before death, of distinguished persons from the beginning of history up to the present time. Only the most reliable sources have been utilized; and even here where there appeared to be any doubt the persons were omitted, so that we have remaining but 794 cases. It must be remembered, however, that very few death-bed experiences are published, and still fewer are described with sufficient accuracy to be made the object of scientific study.

Taking into consideration the very many and varied sources, in all periods of history, which have been consulted, the regularity of the figures in the table is remarkable. Such uniformity, coming out of most heterogeneous conditions, when put into statistical form, suggests that death is a great equalizer and leveler for all humanity. In a way, the conduct and last words of those facing death are a mental and moral test of their real character.

## PSYCHOLOGY OF DEATH OF DISTINGUISHED PERSONS IN HISTORY

Profession or Occupation	Number of Persons	Average Age	Manner of Death					At Death Number		Mental State of Dying Persons																								
			Violence					Little or no Pain	Pain	Last Words At or Just Before Death																								
Executed		Killed in Battle	Suicide	Assassinated	Total	Disease	Number of Persons																											
																				Average Number of Words														
											Religious	Sarcastic, Jocose	Request, Directions Admonition	Question, Answer, Exclamation	Contented	Discontented	More or Less In- different	Religious	Sarcastic, Jocose	Request, Directions Admonition	Question, Answer, Exclamation	Contented	Discontented	More or Less In- different										
											12	13	14	15	16	17	18	19	20	21	22	23	24	25										
											2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
1	192	56	42	5	0	4	46	146	12	3	39	6	26	10	56	17	30	23	17	23	2	6	25	22	19									
Religious.....	91	50	23	0	0	7	37	54	5	4	11	7	28	11	13	16	18	13	10	17	2	6	23	22	13									
Royalty.....	75	51	21	5	5	6	49	26	31	2	2	6	6	14	15	13	17	16	16	6	6	11	16	17										
Military.....	102	60	71	0	1	2	11	58	15	0	2	6	8	14	23	17	9	17	10	16	5	8	11	20										
Philosophers.....	106	64	6	2	5	2	14	56	10	0	9	24	3	9	28	10	27	18	11	14	5	4	15	16										
Literateurs.....																																		
Physicians and Sci- entists.....	54	64	3	0	0	0	3	51	4	4	4	1	8	3	18	3	10	9	0	8	5	23	11	10										
Artists.....	39	63	0	0	0	0	0	39	4	3	3	3	2	8	7	4	9	21	13	0	5	12	16	9										
Poets.....	69	56	3	1	0	5	64	33	4	8	4	8	17	8	17	2	18	20	24	14	3	15	16	15										
Statesmen.....	96	65	18	1	0	6	27	69	6	14	10	26	14	10	26	14	17	14	45	18	3	10	14	12										
Women.....	70	56	29	0	2	0	22	48	7	3	7	5	9	6	17	11	9	12	34	63	8	11	15	15										
Totals	794	60	142	30	15	27	214	580	59	21	93	60	112	87	227	96	170	18	20	19	5	16	16	14										
Total Averages....																																		
Percentages.....																																		
			18	4	2	3	27	73	63	35	26	17	31	26	46	19	35																	



The persons whose records we have studied are classified, according to occupation, into ten divisions, as indicated in the first column of the table. Those whose profession was of a religious character are the largest in number (192), which is due doubtless to the power of religion throughout all history. Under "philosophers" are included mathematicians and educators. As the number of women was not large enough to make subdivisions, all women are placed together.

It will be seen from the third column of the table, which gives the average age, that the great majority of men who become eminent must live at least fifty years. Royalty and Military show the lowest average age, due in part to the large number of deaths by violence, which is the case also with Religious, Statesmen and Women. In short, all the ages in divisions where there are many deaths by violence would of course have a much higher average age had they lived their natural lives.

If we eliminate this factor of death by violence, the Poets and Artists die the youngest; thus Keats died at 26, Byron at 36, Burns at 37, Poe at 38, and Addison at 47. In the columns for pain or little or no pain at death, it will be seen that in only 80 cases out of 794 was any reference made to this matter, indicating that the question of pain at death is regarded as of little importance. In the last 14 columns of the table is presented the mental state at death or just before death, as shown by the last words. It will be noted, from column 13 at the bottom, that 17 percent were sarcastic or jocose, indicating a high degree of mental control. In fact some of the dying complained that it was taking too long and they were getting tired. A relatively large number (24, or 37 percent.) of writers and authors (literateurs) were jocose or sarcastic or both (column 13); they also were relatively the freest from pain (column 10).

The Military show much the relatively highest number of requests, directions or admonitions (column 14) in their last words. The Philosophers stand relatively high in questions, answers and exclamations (column 15). In general it will be noted (columns 12-15) that requests, directions and admonitions were most frequent (31 percent).

More than twice as many (46 percent.) were contented than were discontented (19 percent), as is seen at the end of columns 16 and 17; this accords with the fact that 65 percent had little or no pain, and 35 percent had pain. Thirty five percent were indifferent (column 18); but they all took about the same number of words to express their feelings

(averages, columns 23, 24, 25). While relatively few of the Statesmen and Women were sarcastic (column 13), they took many more words to express themselves (column 20) than the others; the Poets also had as high an average as 24 words. In requests, directions and admonitions the Women show an average of 63 words, which is three times as great as that of any of the others, except Royalty, which has 23 (column 21).

As to expressing contentment or discontentment, the Religious and Royalty used the most words, except for contentment, where the Physicians and Scientists have an average of 22 words (column 23). The Artists and Scientists used the fewest words of all (averages 9, 10) to express their indifference (column 24).

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## MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

Communicated by JOHN E. ANDERSON

### I. AN EXPERIMENT IN TIME ESTIMATION USING DIFFERENT INTERPOLATIONS

By LLEWELLYN T. SPENCER

The problem of the perception of time has been given frequent and almost continuous treatment. Much has been done, especially in Germany, in regard to difference limina, increased uncertainty with increased length of interval, etc. In this country, among other investigators, R. McDougall<sup>1</sup> with thirty subjects found sex differences. He found that women are less accurate than men in their estimates of time-intervals and had a greater tendency to overestimate. Yerkes and Urban<sup>2</sup> conducted a group experiment on a far larger scale, in order to study this difference. Four intervals were used, 18, 36, 72, and 108 sec. The subject recorded his estimation of the interval in seconds. The interval was given by the signals "start" and "stop" spoken by the experimenter. With each of the intervals four different "fillings" or interpolations were used. These were idleness, reading (by the experimenter), writing (at dictation), and estimation (any method except the use of a timepiece). In the experiment approximately two hundred and fifty males and approximately two hundred and fifty females were used. This made a grand total of over 8,400 observations. Of all the observations for all the intervals and interpolations 2.39% were correct for males and 1.05% for females. This bore out McDougall's findings. They also found that the number of correct judgments decreases with the increase in the length of the interval. But upon examining their results, they discovered that in the judgments reported the final digit was most often a multiple of five; 65.9% of the male judgments and 80% of the female judgments ended in 5 or 0. This frequency is too great to be due merely to chance. They also found that even numbers were more frequent than odd numbers as the final digit. The order of decreasing frequency for the last digit was 0, 5, 8, 2, etc. In other words judgments "were strongly influenced by the thought of the conventional time unit, the minute, for in all quantitative work there are errors in favor of the standard of measurement and simple fractions thereof. . . . The longer an interval . . . the more frequently it is judged as the same as the unit or a simple fraction of that unit."<sup>3</sup> Inasmuch as the intervals used were not multiples of 5, it is clear that a certain influence not present in normal behavior affected the judgments of the subjects in the investigation of Yerkes and Urban. The present investigation was undertaken to determine whether a method of judgment which is not

<sup>1</sup> *Science*, N. S. 19, 1904, 708-709.

<sup>2</sup> *Harvard Psychological Studies*, 2, 1906, 405-430.

<sup>3</sup> *Op. cit.*, 417 and 418.

subject to this influence would produce results varying significantly from those obtained by Yerkes and Urban. The method chosen was that of reproduction.

The interval was given by a sharp rap on the table with a pencil or ruler as a signal of the beginning and of the end of the interval. The subject immediately reproduced the interval by starting and stopping a stop-watch, which was enclosed in a felt-lined case to prevent any reference to the rhythmical ticking. In this way the judgment was freed of any prejudice from "the thought of the conventional time unit." The judgments could be obtained to the fifth of a second. Four intervals were used, 15, 30, 60, and 100 sec. These closely approximate those used by Yerkes and Urban. The experiment was divided into two parts. In the first part the four intervals were given in mixed order, without any interpolation. This corresponds to the "filling" called "estimation" by Yerkes and Urban and will hereafter be designated by that name. The subjects were instructed to reproduce the interval immediately after the final rap, without using any

TABLES OF RESULTS

TABLE I. 15 SEC.		TABLE II. 30 SEC.		TABLE III. 60 SEC.		TABLE IV. 100 SEC.	
Estimation		Estimation		Estimation		Estimation	
Sec.	Cases	Sec.	Cases	Sec.	Cases	Sec.	Cases
10-14	12	20-24	9	30-34	1	35-39	1
15-19	43	25-29	18	35-39	2	40-44	1
20-24	17	30-34	22	40-44	2	45-49	1
25-29	11	35-39	17	45-49	7	50-54	2
30-34	5	40-44	8	50-54	9	55-59	1
35-39	4	45-49	11	55-59	27	60-64	3
40-44	3	50-54	5	60-64	11	65-69	1
45-49	1	55-59	4	65-69	7	70-74	4
50-54	1	60-64	2	70-74	7	75-79	2
55-59	2	65-69	1	75-79	6	80-84	9
60-64	1	70-74	1	80-84	7	85-89	7
		75-79	1	85-89	1	90-94	7
		105-109	1	90-94	1	95-99	11
				95-99	4	100-104	7
				100-104	2	105-109	8
				105-109	1	110-114	8
				110-114	1	115-119	7
				115-119	1	120-124	2
				120-124	1	125-129	4
				125-129	1	130-134	1
				130-134	1	135-139	2
				135-139	1	140-144	2
				140-144	1	145-149	3
				145-149	1	150-154	1
				150-154	1	155-159	2
				155-159	1	160-164	1
				160-164	1	170-174	2
				170-174	1	200-204	1
				165-169	1		

method of rhythmic comparison; that is, counting in the stimulus and response intervals, humming tunes, or thinking poetry, and thus gaining a basis for comparison, were forbidden. During the reproduction the subject was idle both in the first set of observations and in the second set, described below. Ten subjects were used throughout the experiment. These were men of the undergraduate body of Yale College and the Sheffield Scientific School and of the Yale Psychological Department. The four intervals were given twice at each sitting and only one sitting was held each day. No attempt was made to keep the sittings regular, but each subject had five sittings on each interval or interpolation, which makes a total of one hundred judgments on each interval or interpolation. As the subjects were ignorant of the success of their judgments, practice-effects could have small influence. Tables I, II, III, and IV give the distribution of judgments for these four intervals in classes of five.

In the second part of the experiment, the 30 sec. interval was used with the following interpolations: Reading of prose by the experimenter, Reading of prose by the subject, Reading of poetry by the experimenter, Reading of poetry by the subject, and Dictation. The interpolation was of course used only in the stimulus interval, and during the reproduction interval the subject spent the time in idleness as in the period of estimation described above. For the prose, selections from current periodicals were used and familiar and especially rhythmical poems were employed in the intervals devoted to poetry. Dictation was from the material of the reading. Table V gives the distribution of judgments for these five interpolations. For convenience of reference, the 30 sec. interval with estimation (idleness) is included also in this table.

TABLE V  
INTERPOLATIONS FOR 30 SECS.

Judgment	Estima- tion	Prose By Experi- mnter	Prose by Subject	Poetry By Experi- mnter	Poetry by Subject	Dictation
5-9						2
10-14			1	1		10
15-19		5	1		2	9
20-24	9	4	6	9	8	10
25-29	18	18	15	5	10	14
30-34	22	24	16	22	16	18
35-39	17	11	19	18	15	8
40-44	8	14	12	24	14	9
45-49	11	9	7	7	13	11
50-54	5	7	11	6	7	3
55-59	4	6	6	3	7	2
60-64	2		2		2	
65-69	1	1	1	3	1	1
70-74	1		2	1	2	2
75-79	1					
80-84			1		1	
85-89					1	
90-94					1	1
95-99		1				
100-104				1		
105-109	1					



Of the nine hundred observations, thirty-five are correct. Responses were read to the nearest second, under the assumption that responses within two-and-one-half fifths either side of the unit second were equally distributed within those limits. This gives a percentage of 3.88% correct. Yerkes and Urban found the percentage to be 2.39%, as stated above.

The effect upon the percentage of correct responses of the prejudice in favor of the multiples of 5 can be approximately calculated. It is evident that this prejudice increases the returns for two of the possible final digits, at the expense of the other eight. It is assumed that this prejudice operates in a fairly regular manner. Such an assumption is always made in the description of any psychological trait. For example, we assume that every time the Müller-Lyer illusion is presented there will be a constant tendency to misjudge the distances. The percentage given by Yerkes and Urban of 65.9% of the responses ending in 5 or 0 represents the expected returns for those digits *increased* by a certain percentage at the expense of the other digits. Of all the responses only 20% should end in 5 or 0, on the basis of pure chance. Hence the difference between the 65.9% obtained and the 20% expected, viz., 45.9%, represents an increase of the returns for 5 or 0, due to the disturbing influence. This percentage should be redistributed among the other eight digits, so that  $45.9\% \div 8 = 5.73\%$  approximately should be allotted to the percentage of cases of the other final digits. I say 'approximately,' for it was shown in their article that the other final digits were also affected by the prejudice, but not by any means to such a great degree. But this addendum of 5.73% refers only to the final digit in each case. We are interested in the correct tens digit as well. Only a portion of this addendum should be allotted to the particular percentage of correct final digits falling within the correct tens. There are obviously cases where the final digit is correct but the tens digit is incorrect. An examination of the data given in the article of Yerkes and Urban shows that 24.86% of the responses were of the correct tens digit. Therefore 24.86% of the addendum should be added to the percentage of cases in which a correct response occurred; and 24.86% of 5.73% is 1.41%. This percentage, then, should be added to the percentage obtained while the prejudice in favor of 5 or 0 was operative. The percentage of correct responses obtained by Yerkes and Urban was 2.39%; and  $2.39\% + 1.41\% = 3.80\%$ . This should give us the correct percentage of correct responses obtained when the prejudice referred to is not present. As a matter of fact, in the present experiment, the percentage of correct responses found was 3.88%. The difference of only .08% between the theoretically calculated percentage and the percentage actually obtained is so small that we are justified in considering that the amounts agree in a striking manner.

It may be objected that the present argument assumes that the judgments were distributed purely by chance, whereas the distribution clearly shows a tendency to judge the interval approximately correctly. It is certainly true that the argument rests upon that assumption. But such an assumption is limited to the distribution *within the tens*. In other words, we assume that, while the ability to judge time-intervals is sufficient to enable us to approximate the correct length, this approximation is only to the correct tens digit and not as close as the final digit. Within the tens the distribution of the responses is assumed to be fairly in accordance with the laws of chance. It is possible to discover from our data whether this is actually the case. An

examination of the results gives the following distribution of responses in terms of the final digits within the correct tens.

Distance of digit from correct digit	-4	-3	-2	-1	0	+1	+2	+3	+4	+5
Number of cases	20	25	30	20	35	41	25	30	38	30

This shows that the distribution is approximately by chance and is by no means grouped about the correct digit. It is therefore justifiable to assume the chance distribution within the correct tens, which is required by the preceding argument. If the calculations are correct, this close correspondence between the theoretically calculated percentage and the percentage actually observed confirms the accuracy of the present experiment, and shows conclusively that the prejudice mentioned did decidedly affect the judgments of the subjects of Yerkes and Urban's experiment. The difference in the percentage of correct responses as reported by the two investigations is therefore clearly traceable to the difference in method.

Using the mean as a measure of central tendency, Yerkes and Urban found that the 18 sec. interval of "estimation" was overestimated (by 1.5 sec.). In the present investigation the 15 sec. interval in estimation is overestimated also (by 7.27 sec.). In this the two experiments agree. For the longer intervals there is a marked difference. For the 36, 72, and 108 sec. intervals of estimation they found an underestimation of 2.9, 8.8, and 0.2 sec. respectively. I find an *overestimation* in the case of the 30, 60, and 100 sec. intervals of 8.23, 8.55, and 4.31 sec. This difference in the direction of error may be a significant function of the methods used. Table VI gives the means mentioned, as well as the medians and measures of variability. All were calculated from the unclassified measures.

TABLE VI

Int. in Sec.	Interp.	Mean	$\sigma$	PEm	Mdn
15	Estimation	22.27	7.79	.52	19.5
60	Estimation	68.55	21.40	1.44	61.1
100	Estimation	104.31	26.35	1.77	100.5
30	Estimation	38.23	12.20	.82	35.2
30	Prose by Exp.	37.25	12.05	.81	34.8
30	Prose by Subj.	38.36	10.89	.73	36.6
30	Poetry by Exp.	39.07	12.25	.82	37.3
30	Poetry by Subj.	40.89	14.21	.85	38.7
30	Dictation	32.41	10.65	.67	30.3

In the course of the experiment it was noticed that a subject would occasionally lose all track of the interval, or would be disturbed by some thought or unavoidable distraction, so that certain responses were without doubt abnormally shortened or prolonged. The judgment of 105 sec. for a 30 sec. interval of estimation is certainly not free from some uncontrolled influence. In consideration of this fact, it seems that a measure, such as the median, which does not consider

the value of the extremes, is a more accurate measure of the central tendency than the mean. Using the median as a measure, the 15 and 30 sec. intervals are seen to be overestimated. The 60 and 100 sec. intervals show no definite overestimation if the median is used.

In summing up, the following points are significant:

1. The method of reproduction produces more accurate results than the method of statement in terms of the standard unit. The error of the latter method in the case of Yerkes and Urban's results was theoretically calculated, and the corrected percentage of correct responses is shown to agree with the percentage obtained by the former method.
2. While the percentage of correct responses is greater when the method of reproduction is used, the percentage thus obtained (3.88%) is very low and shows that time-estimation is not very accurate.
3. Results obtained by the method of reproduction do not agree with those obtained by the method of statement in terms of the standard unit in showing that all intervals of greater length than 18 sec. are underestimated. On the contrary, all intervals were overestimated when the method of reproduction was used. If the median is used as the measure of central tendency, the overestimation of the 60 and 100 sec. intervals is not significant. This is true no matter what interpolations are used.
4. Poetry as an interpolation causes the interval to be estimated as longer than an interval with no interpolation (*i. e.*, estimation) or than one with the reading of prose.
5. The interval is estimated as longer if the subject reads the prose or the poetry than if the material is read by the experimenter.
6. Dictation causes the interval to be estimated as shorter than one with any other of the interpolations used, but not necessarily shorter than the actual interval.
7. The results of Yerkes and Urban agree with this study in the comparison of one interpolation with another, as to the effect upon the judgments, although they vary from the present findings in the relation of the judgments of the intervals to the actual intervals.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LIII. THE INVOLUNTARY RESPONSE TO PLEASANTNESS

By G. H. CORWIN

In a recent paper on *Pleasantness and Unpleasantness in Relation to Organic Response*, Young says:<sup>1</sup> "While U is associated with a large variety of reflex movements, P is passive and negative. Our data do not contain a single case of active reflex response to a P stimulus. With U there are withdrawing movements, frowning, straining, reflexes of expulsion, etc., while P is characterized by mere acceptance of the situation and the passive yielding to it. . . . Muscular strain (tension) is correlated with U, while its opposite, relaxation, is correlated with P.

"The traditional relation between P and seeking movements finds little support in our reports, while that between U and withdrawal is abundantly confirmed. The entire evidence for seeking movements, as 'expressions' of P, is found in 11 reports, in which the subject 'sniffed,' 'took a deep breath in order to get more,' 'held the breath,' etc. In every case these seeking movements are deliberate and never reflex, like most of the withdrawing movements of U. It is a question to what extent and in what sense voluntary deliberate behavior based upon the knowledge that P may be produced or prolonged, or U avoided, can be considered an 'expression of feeling.'"

This result, that the typical involuntary reaction to P is a relaxation and not a seeking movement, is contrary, as Young says, to psychological tradition; and it occurred to us that it might be occasioned by the method which Young employed. In fact, he himself implies that, under other conditions, another result might have been found. "It should be remembered that any result is a function of conditions. Our subjects were seated quietly in a Morris chair, instructed to be 'passive and receptive' and 'to let the experimental situation have its full normal effect.'" We have therefore undertaken in our experiment to put the O in a different situation. Instead of giving him a stimulus under conditions in which a seeking movement would be not only unnecessary but also impossible, and further instructing him to be 'passive and receptive,' we have tried to create a situation in which the O must seek if he desires to retain a P, and also to find an instruction under which he might move without disobeying it.

Our experiments fall into three series according to the stimuli used. We shall designate them as the olfactory, cutaneous and auditory experiments.

*The Olfactory Experiment.* The O's were seated in an ordinary chair in a room especially prepared for the experiment. To the right of the O a Ludwig kymograph was placed upon a table. This was concealed by a grey screen. The revolving drum had been removed, and a wooden rod 58 by 2.8 cm. was screwed to the center of the revolving disk. A clamp was fastened to the free end of the rod in

<sup>1</sup> P. T. Young, this JOURNAL, xxxii, 1921, 38ff.

such fashion that the stimulus-vials could be easily inserted and removed. By means of this apparatus a vial could be placed on the end of the rod directly under *O*'s nostrils; when the kymograph was set in motion, the stimulus would recede from him at the rate of 1.7 cm. per sec. A small electric fan, which served the double function of keeping the air in motion and of eliminating the noise of the kymograph, was operated during this experiment.

As a check upon *O*'s involuntary movements we took graphic records throughout this series. For this purpose a band was placed around *O*'s head and a cord extended to an ergograph which carried a pointer. By this arrangement any backward movement of the head was indicated upon the drum by a line in an upward, and any forward movement by a line in a downward direction.

The *O*'s were Assistant Professor L. B. Hoisington (H), Dr. H. G. Bishop (Bi), and Miss D. B. Dewey (D), a student taking her 'major' in psychology. Despite the fact that H and Bi worked with some knowledge of the problem, H assures us that his results are not influenced by knowledge, and that his reactions were entirely involuntary; whenever Bi suspected that knowledge might have influenced his results, he reported the fact. Every *O* worked two or three hours a week for periods of 40-60 min. The work was done during the spring of 1921. Five other (untrained) *O*'s were occasionally introduced into the experiment as a control. In every case we found positive results similar to, although not as extensive as, those reported by H, Bi, and D.

A preliminary series extending over two weeks was completed in order to determine the most effective stimuli and the best form of instruction. The following stimuli were employed: vanillin and cinnamon (3 to 1); Cashmere bouquet toilet water; whiskey; ol. rutae; carbon bisulphide; asafoetida; and empty vials. The instructions were: "You will be given a series of olfactory stimuli, some of which are *intensive* odors, some have no odors at all. At the signal 'now' smell the stimulus. You are asked (1) to report whether the immediate experience is pleasant, unpleasant or indifferent, and to indicate the *intensity of the feeling* (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong;') and (2) to report all muscular tendencies and organic sensations in any way related to the affective reaction."

From our preliminary series it had become evident that the very instruction 'to smell' might predispose the *O* to continue smelling, and might therefore occasion a pursuit for another reason than *P*. To offset this difficulty and also other predispositions, such as for example, curiosity, interest, the "laboratory attitude," etc., we introduced as stimuli several empty vials, and informed the *O* in the instruction that some of the stimuli would be *intensive* odors and some would have no odor at all. It turned out, however, that whenever an *O* actually moved or had a tendency to move from any of these extraneous motives, the fact was always reported in his introspections.

We give in Table I a summary of our results in quantitative form. It will be observed that for all *O*'s pursuit movements occurred in about one-half of the cases in which *P* was reported. Furthermore, for H and D, in addition to definite pursuit tendencies or movements, there was a much larger proportion of characteristic reactions which indicated their desire to prolong or 'get more of' the stimulus. H



reports organic stir with P in 17 cases; it is almost never found in the reports of Bi and D. These O's, however, were in 9 and 17 cases respectively actually seen by E to make gross pursuit movements.

TABLE I

Observer	H	Bi	D
Total No. Reports.....	73	46	60
Total No. Reports classified as P.....	36	26	42
No. of P reports characterized by definite seeking movements or tendencies to movement.....	15	13	18
No. of P reports characterized by secondary seeking movements, such as: sniffing, raising shoulders, change in breathing, extending arms, etc.....	34	11	28
No. of P reports with organic stir.....	17	0	5
No. of P reports with gross observed movement.....	0	9	17

Examples of the kind of introspection that we have classified in line 4 of the table (definite seeking movements) are:

D. "I felt attracted toward it and then repelled as it [the stimulus] became U." "Pushing of the shoulders and head forward." "Straining of the trunk upward." "Relaxing of my trunk towards the smell." "Moderately P, and I was inclined to follow it." "Tendency to go toward the stimulus not in a relaxed way but in a tense impulse." "I felt a tendency to move toward the stimulus."

Bi. "I felt a general pressure-kinaesthesia that would favor following up the odor, and when the stimulus moved away I felt also a decided pressure in my thigh-muscles. Knowing what I do about the experiment, I am very careful not to read any movement into it. This definite movement of pursuit, however, quite surprised me; it was just as sharply automatic as the movement which one makes when his hat blows off. When the stimulus suddenly disappeared I suddenly felt myself in pursuit." "That's asafoetida! I liked it at first, and then it became U. Both were weak. I seemed to follow it at first and then, when it became U, I stopped." [He followed it and then jerked back.] "I felt muscular sensation in my head as I followed." "I felt like staying with that, keeping right after it."

H. "A general tendency to strain forward which was different from the other sort of expansive leaning." "Tendency to lean." "Tendency to react forward." "As it weakened there was a tendency to try to get more of the odor and there was a tendency to lean over. I must have leaned." "There seemed to be a bodily set of getting the odor; of going after the odor." "Thought I noticed an impulsion to get more of the experience."

Examples of line 5 (secondary movements) are:

D. "Relaxation of the head." "Relaxation of the neck." "Increase in saliva." "My shoulders raised with the inhalation." "Felt my trunk relax toward the stimulus." "Took long breaths to get all the sensation possible." "Tendency to take quick breaths." "Tendency to sit up towards the stimulus."

Bi. "Certain tendencies to keep on sniffing." "Just an easy relaxed kind of P." "One continuous uniform inhalation." "Just a free comfortable inhalation." "Pleasant relaxation all through the body. I had the relaxed, unrestrained breathing that comes with P." "Whole lot of sniffing movements. Kinaesthesia in the throat. Free nostrils." "A feeling in my face, as when I am just about to smile. These facial reactions were directly related to the P." "Bodily relaxation and comfortableness."

H. "A rather expansive tendency, that is, to throw the diaphragm down." "Slight tendency to deeper inhalation." "That same tendency to deeper, prolonged inhalation and rather more rapid expiration." "Certainly a muscular relaxing effect, very different from the experience one gets with U." "Seemed to be a tendency to get more of the odor." "Deep breathing. This sets up certain kinds of pressure in the abdominal region; not very strong in intensity. They have a lively something about them." "There seemed to be a tendency to a general muscular, I was going to say, relaxation, yet not in the sense of going flabby. The tonus of the muscles seems to stay rather high and yet not to constrict." "A peculiar muscular tendency in the direction of relaxation but with a fair degree of tonus." "There is an expansive effect different from relaxation, for there is still a high degree of muscular tonus, the muscles do not go flabby, there is a sort of brightness, a kind of glow to it." "There was muscular relaxation; I don't like that word 'relaxation' because the experience is as much a contraction as it is relaxation; perhaps a different set of muscles is involved. It may be characterized as 'expansive;' a 'glow.'" "There was certainly a muscular tendency both to relaxation and expansion (not going flabby). There is apparently a qualitative difference between the pressures when muscles contract and when they relax. There seemed to be an urge to get more of the stimulus." [E asked H to explain his statement about the muscles. H replied that contraction was correlated with U, it had a dull, uniformly intense nature. The experience was more or less definitely localized and definitely limited. Extension was correlated with P, and had a bright quality. It was more generally diffuse, was very indefinitely limited, and its intensity fluctuated within narrow limits.]

Examples of line 6 (organic stir) are:

D. "Made me feel hungry, empty feeling in my stomach." "It made pressure on the wall of the stomach." "I felt contraction of the stomach."

H. "There was a weak organic sensation in the region of the stomach, still there, diffuse, not definitely localized." "A bit of fleeting organic." "Slight pressure in the abdominal region." "Something like organic that eludes me all the time. Rather a bright-like pressure." "Weak organic something in stomach region, very indefinitely localized."

*The Cutaneous Experiment.* This experiment was conducted in the same manner as the Olfactory Experiment except that the first sentence of the instruction was made appropriate to the change in stimuli; in place of odors, five cutaneous stimuli were used. These stimuli were a draught of cool air, cold iron, a board containing fine sharp points, a board covered with sand-paper, and a board covered with soft fur. These were moved slowly across O's forehead or nose.

It will be noted from Table II that, although the series is short, H found decided movement or tendencies to movement with *every* P

experience; also that Bi again reported no organic stir with P. It will also be seen that what we have classified as secondary characteristics of P were less marked and primary characteristics were more marked in this than in the olfactory experiment. There was only one case reported of a change in respiration.

TABLE II

Observer	H	Bi	D
Total No. of Reports.....	16	13	10
Total No. of Reports classified as P.....	9	8	5
No. of P reports characterized by definite seeking movements or tendencies to movement.....	9	4	4
No. of P reports characterized by secondary seeking movements, such as expanding, change in breathing, changing from nose to whole cheek, etc.....	6	1	2
No. of P reports with organic stir.....	5	0	2
No. of P reports with gross observed movement .....	4	3	3

Examples of the kind of introspection that we have classified in line 4 of the table are:

D. "Tendency to follow the stimulus in order to keep it next to the neck."

Bi. "I did have a tendency to put my cheek on it and follow it." "A little follow-up tendency."

H. "Then there was a positive muscular tendency forward." "There was a tendency to lean forward a bit so that the sensation would be less of a ticklish sort, but a rubbing with a fair degree of pressure." "I did not realize how P it was until it was over. When the support was removed I toppled over." "Same tendency to lean toward the stimulus and to maintain movement." "The stimulus is more at your command here. It is there, you know it is there, and you can do with it what you want. Quite a low level of attention anyhow."

Examples of line 5 are:

D. "Organism seems to expand and to grow tall towards it." "There was a tendency to take long breaths."

Bi. "I changed from nose to cheek."

H. "There was almost a tendency to lean and to expand upward." "There was a muscular tendency of relaxation; it becomes almost a desire to maintain the experience."

Examples of line 6 are:

D. "There was a general feeling of P throughout the entire body."

H. "There was a general liveliness of pressure-quality, an expansive tendency in the abdominal region; the whole muscular system from the hips up was involved in it."

*The Auditory Experiment.* The same instruction was used in this experiment, except that the first sentence was again made appropriate to the change in stimuli.

Two adjoining rooms were used. In *E*'s room there was a large funnel-shaped horn. From the small end of this horn a 1.25 cm. rubber tube extended through double doors into *O*'s room. While one *E* operated the stimuli, a second *E* moved the free end of the tube gradually away from *O*'s ear.

The stimuli were: pleasing musical selections (victrola records); an unpleasant record made by causing the record to revolve from a point other than the center; chords and discords played on tuning forks; the scratching of the finger nail on sand paper; and the dropping of brass rings.

During the last half of this experiment the *O*'s were definitely instructed "voluntarily to inhibit any movement or tendencies to movement." The introspections marked \* were taken from reports under this negative instruction. It will be noted from Table III that Bi reported marked tendencies to movement or actual movement in 14 out of 16 P cases; it is also interesting to note that here he reports two cases with organic stir.

TABLE III

Observer	H	Bi	D
Total No. of Reports.....	35	31	35
Total No. of Reports classified as P.....	22	16	28
No. of P reports characterized by definite seeking movements or tendencies to movement.....	15	14	15
No. of P reports characterized by secondary seeking movements, such as change in breathing, expansion, etc.....	17	6	23
No. of P reports with organic stir.....	12	2	15
No. of P reports with gross observed movement .....	12	8	12

Examples of the kind of introspection that we have classified in line 4 of the table are:

D. "Inclination to follow stimulus." \* "Inclination to follow stimulus was repressed because of instruction." "There was a tendency to move the trunk in order to follow the stimulus as it moved in different directions, going away from me and then returning."

Bi. "There was pursuit from two causes, from P of the music itself, and from the effort to hear every note in order to identify the selection." "I felt strain in my neck as if I were following the tube backwards and upwards." "That was very P. The only U part was being compelled to pursue the tube in order to maintain the P experience." \* "I found myself wishing that I could stretch out my ear. I am sure that my eyes turned in the direction of the stimulus two or three times. I am sure that I found myself starting to move." \* "I could feel a pretty marked incipient tendency toward movement in order to hear the tone better." \* "Once or twice there was a definite tendency to incline toward the door. This was when the music became weak." \* "I had a great many ideas of movement of pursuit or tendencies to pursuit. Probably these were ideated movements and

are just pictorial of what I should have to do in order to follow the stimulus if actual movement were not inhibited."

H. "There was a special tendency to reach toward the stimulus when it became very weak." "The dying out of the tone brought an impulse to move head, this movement was noted after its beginning." "There was a muscular impulse to turn toward or adjust for the having of the experience." "I had an impulse to turn toward the sound source." \* "There was a tendency to move my head which was corrected after the movement was made."

Examples of line 5 are:

D. "I took long breaths and relaxed." "There was a tingling in my finger tips. I closed my eyes." "I swayed." "There was a certain amount of rhythmical kinaesthesia." "There was a tendency to take deep inhalations. I always have this tendency with P."

Bi. "There was a tension in my neck of the kind that would turn my ear toward the tube." "There was a P kind of kinaesthesia." "Two or three times I found a tendency to rhythm."

H. "There was relaxation with slight expansion and deep easy breathing." "I breathed rhythmically." "There was an expansive tendency."

Examples of line 6 are:

D. "I experienced a sinking sensation in the pit of my stomach." "My organic system was very relaxed." "My stomach felt heavy."

Bi. "There was perhaps a slight organic 'stir' of P." "There was a kind of organic thrill."

H. "I experienced a diffuse quality that was bright and lively." "There was a diffuse bright pressure." "There was a diffuse expansive tendency in the abdominal region especially, but also in the body in general, a very weak bright-like pressure."

#### *General Conclusions*

We find that in 55.7% of the total number of cases classified as P all our O's reported definite movement or tendencies to move; while in 66.6% of the cases secondary reactions characteristic of pursuit or desire 'to maintain the experience' were also recorded. When we note that 28.6% of the cases reporting secondary seeking characteristics were found in P cases other than those classified in line 4, we therefore determine that definitely seeking or maintaining reactions to P stimulation are found in 84.3% of the total number of P cases; whereas Young found no direct involuntary tendencies to movement, or actual movement, reports only 11 cases of the secondary type, and classifies even these as voluntary.

The results obtained from our introspections are completely verified by the graphic records taken during the Olfactory Experiment. We noted, however, that the upward direction (with U-withdrawal) took place immediately, and was steeper than the downward direction. The lines correlated with P were of a gradually sloping nature. This result verifies Young's conclusion that the reaction to U is quicker and more intense than that to P. As the O's varied widely in the intensity of their reaction to the various stimuli, a quantitative statement of the amount of change would be inadequate. In general, the younger O's made more pronounced reaction to both U and P stimulation than the older. Since these reactions were entirely involuntary, there can be no doubt that they are characteristic of P, and were not occasioned by instruction or by laboratory predisposition.



In the case of taste, it is an everyday fact that the stimulus is ejected if U and retained if P. Young does not tell us the results of his chocolate-drop experiment.

It seems from our experiment that, if 'contraction' is a better term for describing the involuntary response to U, 'expansion' is a better term for describing the involuntary response to P. With intense U we found a definite withdrawal, and with weaker U a 'shrinking' of the organism, a desire 'to roll up into a ball,' a 'contraction,' a 'dull pressure quality,' etc. These organic tendencies all carried the meaning of withdrawal. Likewise on the side of P we found that there was either (1) actual pursuit or tendency to pursuit, if the stimulus was sufficiently intensive and there was danger of losing it; or (2), if the stimulus was weak or there was no danger of losing it, an expansion of the organism, a desire 'to get more of the experience,' 'to relax toward the stimulus,' 'to relaxation, yet with a rather high degree of muscular tonus,' etc. H has characterized his organic sensations as bright-like, lively, glowing. All these organic sensations carry the meaning of pursuit. In both P and U the intensity of the affective quality is an important determinant of the expression which the response will show.

We believe, therefore, that Young's results are derived from the particular situation under which he placed his O's. As they were seated throughout his experiment in a comfortable chair, they were already relaxed, and therefore it was easier for them to be 'passive and receptive' with P than with U stimuli. Their bodily comfort was a source of constant P stimulation, whereas our O's were indifferent to their position.

A second and more important criticism of Young's method is its inadequacy for producing P responses. If an O has a P stimulus placed under his nostrils, there is no incentive for him to make seeking movements, unless E begins to withdraw the stimulus. Indeed, when we repeated the Olfactory Experiment under Young's conditions and instructions, we found no definite seeking movements or tendencies to move; though the relaxation reported always had an expansive component.

There is no doubt that the most natural response to U is a movement of withdrawal. The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion*, if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite activity of pursuit or of tendencies to pursuit.

#### LIV. THE INTEGRATION OF PUNCTIFORM WARMTH AND PRESSURE

By R. S. MALMUD

This study is a companion to the investigations made by Cutolo<sup>1</sup> and Tung.<sup>2</sup> Cutolo determined the psychological result of the simultaneous stimulation of warm and cold spots; Tung did the same thing for cold and pressure spots; and we have extended the inquiry to warm and pressure spots.

The method of this experiment was, in general, that of Tung, and falls into two parts. In the first part we stimulated a spot that was responsive both to warmth and pressure, and asked our *O*'s to report the course of the subsequent experience. In the second part we first aroused a warmth (without pressure) and then excited a neighboring pressure spot, and asked our *O*'s as before to report the course of the resulting experience. As apparatus for the former we employed the thermaesthesiometer used by Tung, but mounted on a mechanical stimulator instead of Thunberg's spring-balance device, and with warm instead of cold water running through the point. The source of the warm water was a tank about 1m. above the apparatus, heated by a Bunsen burner, and connected by a syphon with another tank of cold water; the syphon served the double purpose of permitting a smaller body of heated water, and at the same time of maintaining it at a fairly constant temperature by adding cold water at the same rate as the heated water flowed out. Thermometers were placed both in the heated tank and at the outlet; by these means we were able to determine the loss in temperature between source and outlet; and knowing the total length of the conduit and the position of the thermaesthesiometer within it, we were able to calculate accurately enough for our purposes the temperature of the water at the stimulus. This was on the average found to be 40.5° C, with a range from 38.5° C to 43° C. The temperature of the research room varied between 21° C and 24.5° C. We worked on the skin of the forearm; four or five reliable warm spots were marked by tattooing, and in an experiment the arm rested in a plaster cast. The instruction given to the *O* was as follows: "You will be given a cutaneous stimulus, and you are asked to report the course of the resulting experience particularly as regards its quality and intensity."

As apparatus for the second part of our experiment we used the electric thermaesthesiometer devised by Dimmick for his experiments on the localization of warmth without pressure.<sup>3</sup> This was fastened in the mechanical stimulator, and in an experiment was lowered over a warm spot to a distance of about 5 mm. above the skin; and then, when warmth was reported, was raised 5 or 10 mm. to avoid the arousal of heat. For the pressure stimulus we had intended to use a von Frey hair; but because the radiant heat so curled the hair as to make it useless, we substituted a wooden cylinder, 2.6 gr. in weight, and with a smooth spherical point about 0.5 mm. in diameter. This,

<sup>1</sup> F. Cutolo, Jr., this JOURNAL, xxix, 1918, 442.

<sup>2</sup> S. Tung, *ib.*, xxxii, 1921, 421.

<sup>3</sup> F. L. Dimmick, this JOURNAL, xxvi, 1915, 142-157.

held loosely in *E*'s hand, was applied over a hair-bulb adjacent to the warm spot. Since the crucial part of an experiment was the moment when pressure was added to the warmth, we first aroused the warmth, then gave the signal 'now,' and immediately thereafter applied the pressure stimulus. The instruction was: "After the signal 'Now' report any change in the quality or intensity of the experience." The *O*'s were Professor Weld (*We*), Mrs. A. K. Whitchurch (*Wh*), a graduate student, and I. Bershansky (*B*), an undergraduate specializing in psychology.

*Results of the First Method.* When a spot on the cutaneous surface that responded both to warmth and to pressure was stimulated with a warm-pressure stimulus, the general course of the experience as described by all *O*'s was, first, a weak pressure (contact), and then a diffuse warmth which with fluctuations gradually disappeared. At times the pressure faded out as warmth appeared; at other times pressure and warmth were present together. In the latter case the pressure was localized either as a core within an area of warmth, or as of the same area and place as the warmth. At times also one of the qualities was focal and the other marginal, and the one focal quality fluctuated in intensity independently of the other; at others both qualities were focal, and they fluctuated together in intensity. When the latter experience occurred, the two qualities were reported as fused, and were characterized as a warm-pressure, or a pressure-warmth. Since these fusions were found also in the second series of our experiments, we shall reserve their further description until we have discussed the results of that part of our investigation.

*Results of the Second Method.* The course of the experience when a warm spot was first excited without touching the skin, and then pressure added by stimulating a neighboring pressure spot, was in general the same as that obtained by the first procedure. The fact that in these experiments the warmth was already focal when pressure appeared seems to have had no influence upon frequency of fusion. Since, in Tung's experiment, the fusion of cold and pressure appeared more readily when attention was directed to the cold, this result of ours was contrary to expectation. Another difference in this series, and one resulting directly from the change in method, was that of relative localization. In some instances pressure was felt at the side of the warmth, and in others entirely without the area of warmth; in still others, however, it was localized, as in the first experiments, either as a core in the center of warmth, or as coincident in area and place with the warmth. In the first two of these cases fusion never occurred.

*The Conditions and Nature of the Fusions.* From an analysis of the introspective reports from both series of experiments it seems that fusion never takes place unless both pressure and warmth are of the same degree of clearness, and unless both are localized in the same area, although pressure may be less in extent than warmth. In some (and perhaps in all) cases of fusion, the components fluctuate or decrease together in intensity; a change in the intensity of the one or the other of the two qualitative components disintegrates the fusion. As we have seen above, the fusions are of two kinds: a warm-pressure, and a pressure-warmth. In the former, either a small core of pressure seems to radiate the warmth, or if the areas are coextensive the pressure is more intensive; it has, furthermore, the object-meaning of "being touched by a warm blunt object." In the latter, the areas are

always coextensive and the warmth is more intensive than the pressure; and it has the object meaning "of warmth pressing down," or "a large, warm, diffuse, yielding body touching the skin." Typical descriptions of the two kinds of fusion follow.

*Warm-pressure. Wh.* "There was a time at the beginning of the experience when pressure and warmth fused; there was a core of hard pressure from which warmth radiated; it was as if a large hard thing were touching me." "Then they [warmth and pressure] fused; it was like a large warm blunt point pressing." "A big warm blunt thing pressing; then both warmth and pressure decreased in intensity, and then suddenly, instead of having the blunt warm thing touching, I had a warm area with a very weak pressure in the center."

*We.* "Thereafter the pressure was felt as a warm-pressure; by that I mean a pressure to which the warmth seems to adhere as its quality; both are there together, but the pressure is the more intensive." "Then the two qualities fused to a warm-pressure; the experience was as if I were touched by a warm object."

*B.* "Pressure and warmth were at the beginning fused; both had the form of a pin-head but were larger. After the pressure had disappeared the warmth became more filmy and diffuse, and without definite form."

*Pressure-warmth. Wh.* "Pressure and warmth were felt simultaneously; pressure seemed to cover the area of the warmth instead of being in the center; it was a fusion, as if warmth were pressing." "Pressure seemed as large as the warmth; there seemed to be a fusion of the two, and the intensity of both diminished and faded out together without fluctuation; the pressure that at the beginning was small fused with the larger warmth when the latter came."

*We.* "The warmth had an insistence about it that was like a deep pressure, weak in intensity; a pressing warmth." "I can characterize it only as like a warmth from a warm point; i.e., a warmth that was pressure in quality; it was not a warm pressure, it was not as if I were touched by a warm object."

*B.* "Pressure seemed to fuse with the warmth which became intense but diffuse and filmy, and seemed to flow out of the skin." "The contact seemed like a big warm thing touching me; the pressure got weaker, but warmth remained."

Of equal importance for determining the conditions of fusion are introspections which describe cases of no-fusion. *Wh.*, for example, says: "The pressure was spatially unrelated to the warmth; I could not localize the pressure within the warmth; they were two separate experiences, temporally but not spatially related;" and *We* likewise reports: "The contact was at one side of the area of warmth; it inhibited the warmth; but when I searched for the latter it was there at very good intensity; the two impressions were not experienced together;" and again: "Pressure is not felt as a warm-pressure, but rather as a pure pressure within an area of warmth."

The conclusion that the fusion of warmth and pressure is only a pressure-warmth or a warm-pressure was, at first, reached by us with some consternation. Since Tung had found the fusion of cold and pressure to be cold-wetness, we had confidently expected to find warm-wetness. This expectation was further justified by a result of Bentley's, who in experiments with areal warmth and pressure had obtained a few reports of warm-wetness.<sup>4</sup> In the face of our own find-

<sup>4</sup> M. Bentley, this JOURNAL, xi, 1900, 414ff.



ing it occurred to us that perhaps our *O*'s did not know the experience of warm-wetness. We, therefore, with two of our experienced *O*'s performed a few simple experiments like dipping the finger into warm water, or causing a drop of warm water to fall upon the back of the hand; but our *O*'s still failed to report warm-wet; they had, it is true, a perception of liquidity, they 'knew' that the finger was immersed in water and must therefore be wet; but there was no experience of wetness; and the only experience from the drops of warm water was warmth and contact. We then, with four *O*'s (Professor Hoisington was added to our group), repeated the significant parts of Bentley's experiment. His conditions were, we believe, duplicated in essential details. Our *O*'s were asked to close their eyes during the time the stimuli were in the room, and after signals to report the qualitative nature of the experience felt at each one of the three stages of the experiment. The results are contradictory. Two *O*'s, *Wc* and *H*, still fail to describe any experience as warm-wetness; the other two, *Wh* and *B*, report warm-wetness frequently. From a study of these reports it is plain, however, that the experiences which carried the meaning of warm-wetness, for the two latter *O*'s, are not the same. For *B*, when the finger is immersed in warm water (39° C.), warm wet comes immediately; is stable except with 'shift of attention,' when it changes to 'dry' warmth; is localized wherever warmth is localized; is felt when finger or stimulus is not moving; and is not distinguished from liquidity. Whereas for *Wh*, under the same conditions, warm-wet does not come immediately; it is uncertain, instable, has short duration, and fluctuates; it is localized under the finger-nail or on the ball of the finger, or again, as creeping up the finger; it is felt only with movement of the stimulus or of the finger; and, finally, it is distinguished from liquidity as 'less thick.' Our only solution of this diversity of report is that *B* has fallen into the stimulus-error, and that *Wh* gives the meaning of warm-wet to some peculiar or atypical experience. In view of the reports of the other two *O*'s, which are in complete agreement, and also in the light of our main experimentation, we incline to the opinion that there is no typical experience to which the meaning of warm-wet attaches, as there is for cold-wet.<sup>5</sup>

*Summary.* The simultaneous stimulation of warmth and pressure spots may or may not result in a fusion of the qualities of warmth and pressure. Fusion occurs when the two qualities are equally clear and, at the same time, are spatially coincident. The fusions are characterized either as warm-pressures or as pressure-warmths. The adjectival quality is the less intensive, and seems to adhere to (or to form a constituent of) the other.

There seems to be no typical experience of warm-wet.

<sup>5</sup> Bentley's *O*'s did not, apparently, distinguish wetness from liquidity. See *op. cit.*, 424.



## BOOK REVIEWS

*Erlebtes und Erkanntes.* By WILHELM WUNDT. Stuttgart, A. Kröner, 1920. Pp. xii., 399.

What a man has made of his life, Wundt says, is measured by the ratio of *Erkanntes* to *Erlebtes*, of the lessons learned from experience to the data of experience itself. The measure, taken in the rather special sense in which Wundt intends it, gives him a sufficiently large vital quotient; few of us would think of using a box on the ear, received in the first year of schooling, to illustrate the Law of the Continuity of Consciousness! Throughout the book, indeed, *Erkanntes* is far more in evidence than *Erlebtes*, and again and again the reader is diverted from some simple item of narrative to a technical disquisition which contains all the subtlety of Wundt's mature thought and shows all the intricacy of his mature style. Since we are familiar with the contents of Wundt's principal works, I shall pass over these scientific homilies and shall confine myself in the present notice to matters of autobiographical fact and to certain questions of genesis.

First of all, however, I must remark that whatever else Wundt learned in the course of his long life he had not learned fully to know himself. He tells us in the Preface that the cast of his mind was political, and its leading motive a desire to further the interests of state and society. There is, no doubt, a grain of truth in this characterization; Wundt was neither the typical man of science nor the typical philosopher. In the main, however, the statement derives from a very natural illusion. Wundt's boyhood was a time of marked political disturbance; he had worked off the superfluous energy of his early manhood in various social and political undertakings; and the closing years of his life were spent in the shadow of the Great War. The end thus harked back to the beginning, and the Karlsruhe episode took on in memory an undue importance. The illusion is, surely, natural enough,<sup>1</sup> and furnishes us with some thirty pages of interesting and instructive reading. It is, none the less, an illusion, and one to which Wundt himself elsewhere supplies the needed corrective. Every man, he declares, really lives a number of lives, which are more or less closely connected, but which run their separate courses to the end. His own political bent belongs, without question, to a partial and subordinate life whose brief periods of dominance are separated by long stretches of scholarly absorption.

In a recent paper I gave a scanty outline of Wundt's life. This outline can now be filled out, and at one point, where I made an unlucky guess, corrected. At the time when Wundt's schooldays began his parents were living at Heidelberg. Here he passed two years in the *Volksschule*, and thereafter, until he entered the *Gymnasium*, was taught privately by his father's *Vikarius*. Wundt was a solitary and imaginative boy, averse from games and from school-work, much given to day-dreaming and, later on, to miscellaneous reading of poetry and historical novels. He was the fourth child in the family; but two children had died so early that he had no remembrance of

<sup>1</sup> Readers of the Leibniz-brochure of 1917 will not think it fanciful if I suggest, as a secondary source of Wundt's belief, the parallel between himself and Leibniz.

them, and his surviving brother, eight years older than himself, was away at school, a member of his aunt's household in Heidelberg. He thus passed his early years "ohne Geschwister und ohne Mitschüler."

His tutor, to whom he was so deeply attached that he followed him for a year to Münzesheim, seems to have been anything but strict and to have taught him but little. Wundt was able, nevertheless, to enter the *Gymnasium* at the neighboring town of Bruchsal, where he spent an unhappy year,—whence, indeed, he ran away, only to be escorted back by his mother. The misfit was so plain, however, that his parents determined to remove him to Heidelberg. Here (and not, as I had guessed, at Mannheim) he attended the *Gymnasium* from his fourteenth year until the time came to exchange school for university. The year of entry was overcast by the death of his father; the later years passed happily. Wundt was taken in hand by his brother, who saw to it that he worked with some steadiness; he made friends, and was pledged to membership in a student *Korps*; following the example of his uncle Arnold, a professor in Tübingen, he devoted himself enthusiastically to the study of the anatomy of the brain; he read widely, and on long walks with his fellows discussed what he had read; in a word, he lived the normal, companionable life of his age and environment.

Required, now, to make choice of a career, Wundt after some hesitation decided upon medicine. He had no special call; but he wanted above all things to get away from home; he had been interested, as we have seen, in brain anatomy; and as a beginning student of medicine he could, with some show of reason, migrate to Tübingen for his uncle's instruction. The year 1851-2 saw him, accordingly, a student at that university. In the summer of 1851 he returned to Heidelberg, with increased knowledge of brain anatomy but with very little beside, save the conviction that nature had not meant him to be a physician. He was forced afresh to a decision, and his choice fell on the new experimental science of physiology. His first Heidelberg year was spent upon preparatory studies in mathematics, physics and chemistry. Bunsen's lectures had well-nigh seduced him into preferring chemistry to physiology; but he remained true to his considered choice, and compromised only in so far that his first scientific publication—duly listed in the JOURNAL's bibliography under the year 1853—dealt with a problem in physiological chemistry. During the second year he studied clinical pathology and pathological anatomy. A prize-essay, in preparation for which he performed, at home and with the help of his mother, a series of vivisectional experiments on the rabbit, brought him a word of commendation from Johannes Müller. The third year—there must have been a third medical year, if later dates are to square—seems to have been devoted to the special medical subjects; I cannot make out that Wundt marks it off, definitely from the year preceding. At the end of this third year, as it would appear, he took his *Staatsexamen*, and came out first in all three divisions: internal medicine, surgery and obstetrics.

In spite of his success in examination, Wundt was still not disposed to attempt a medical practice. He accepted a semester's position as substitute assistant in Hasse's Clinic, and became so keenly interested in his work there that he again had thoughts of deserting physiology, this time for pathological anatomy. In addition to his regular duties, he prepared an experimental thesis for the doctorate of medicine at the University: it was published in 1856. At the expiry of his term of service he determined, with a little money in hand, to go on a

physiological pilgrimage to Berlin, and there to utilize a final semester in work with Johannes Müller and du Bois-Reymond. He entered the University of Berlin in the spring of 1856; began at once a couple of experimental investigations, the results of one of which led to publication; and came back to Heidelberg, his student-days over, in the late summer of the same year.

Wundt's next step was to offer himself as *Privatdozent* in physiology to the medical faculty of his own university. There was very little formality in the matter: the doctorate thesis was accepted as *Habilitationsschrift*, and examinations were dispensed with. He straightway advertised a six-hour course in physiology, with demonstrations and experiments, which attracted an audience of four. But he had undertaken too much. A series of hemorrhages cut short the lectures and the beginnings of research; physicians and family despaired of his life; and it was a full year before his health was thoroughly restored. His experiences and reflections during this illness had a permanent effect upon his philosophy and his idea of the relation of philosophy to science.<sup>2</sup> When he was able to take up his work again, apparently early in 1857, he busied himself with an experimental study of muscular contraction, in continuation of an *Arbeit* suggested by du Bois and begun in Berlin. The book, Wundt's first book, came out in 1858,—dedicated to du Bois, published by Vieweg, "mit vorzüglichen Abbildungen der Apparate und Methoden ausgestattet;" outwardly as inwardly a joy to the heart of its young author. But books have their fates: and this book was destined, since it did not please du Bois, to issue stillborn from the press. At first, Wundt says, he was distressed; then, resigned. Later he began to see the humor of the situation, and later still he forgot the whole affair. Now, in his old age, he reviews his early work, and finds it, on the whole, very good.

When Helmholtz was called to Heidelberg in 1858, Wundt offered himself as assistant, and was accepted. Neither Helmholtz nor Wundt had any very precise idea of what the duties of an assistant might be; Helmholtz vaguely suggested microscopy, which Wundt did not affect. The Government presently cut the difficulty by requiring all students of medicine to take a semester's work in the physiological laboratory; and Wundt accordingly found his mornings, from 8 to 12, given over to elementary instruction and indifferent students. He held out for some years, but finally resigned the position (the story of his dismissal by Helmholtz is pure myth) to devote himself to the writing of his *Physiology and Medical Physics*. As to his relations to Helmholtz, it is hardly an exaggeration to say that they were non-existent. Helmholtz was as silent in fact as the great Moltke is in legend, and Wundt, the assistant, did not venture to ask his professor questions; and though Helmholtz unbent in society, he unbent for social relaxation, and not for the discussion of scientific problems. It was unfortunate, too, that both Helmholtz and Wundt were now occupied with the same subject, sense-perception. Wundt in two passages comments a little regretfully on this coincidence; and there can be no doubt that the community of interest, taken together with a radical difference of perspective, held the two men apart. So we have a few anecdotes, and we have a sort of apology for Helmholtz' liking of foreigners, and that is all.

<sup>2</sup> The incident is discussed by W. Wirth, in the memorial article in *Arch. f. d. g. Psych.*, xl. I regret that I cannot say more of it in this place.



The biographies state that Wundt gave up his assistantship in 1864 and was promoted to *extraordinarius* in the same year. Unless I have somewhere missed a sentence, the date is not mentioned by Wundt himself. He tells us that the *Beiträge* were inspired, primarily by E. H. Weber and by his own experiences of cutaneous anaesthesia in Hasse's Clinic, secondarily and more remotely by Johannes Müller. The notion of creative synthesis came to him, in connection with the problem of visual space perception, in the spring of 1858. The genesis of the *Physiologische Psychologie* is less clear, and Wundt's account of the book itself is, I venture to think, somewhat colored by his later views,—though this coloring, since it shows us what Wundt would have planned if he were writing the work anew, is of importance in its own right. The one thing we see clearly is that Wundt, during the time of his connection with Helmholtz, was steadily reading the standard psychologies of the day. He got up at 5 o'clock in winter to study Herbart's *Psychologie als Wissenschaft*; and he familiarized himself with Lotze, Fortlage, George, Volkman, and the rest, as well as with the older psychology of Wolff and Kant. If this wider interest appears, in the light of Wundt's whole career, as a natural consequence of the original interest in sense-perception, I must still believe, as I suggested in the paper already referred to, that it reflects also a direct reaction against the anti-psychologism of Helmholtz. Otherwise we should have heard more of psychological difficulties, and more of the actual motivation of the *Physiologische Psychologie*. The book came out, as we know, in 1873-4, and its successive revisions are described by Wundt as "ein beträchtlicher Teil meiner Lebensarbeit."

We have, I take it, a like admixture of *Wahrheit und Dichtung* in the account of the genesis of the *Völkpsychologie*. We need not question the statement that, about the year 1860, Wundt conceived the idea of adding social psychology as a sort of superstructure to experimental psychology; the preface to the *Vorlesungen* stands as witness. But Wundt in 1920 had not gone over this work as carefully as he had restudied the *Muskelbewegung*. He forgets that the theory of thought is completed in the first volume, before social psychology makes its appearance; he forgets his express repudiation of Lazarus and Steinthal; he forgets his insistence that the book is a book of individual psychology, and that he introduces the facts of social psychology only to round out this individual psychology, especially on the side of morals and religion. The fact remains, of course,—there is *Wahrheit* amid the *Dichtung*,—that the second volume of the *Vorlesungen* is largely made up of material which was later psychologized in the *Völkpsychologie*, and that it contains the germs of later theories. Wundt's first lectures on social psychology were, however, delivered at Zurich in the summer of 1875, after the publication of the *Physiologische Psychologie*; and the *Vorlesungen* probably drew less upon special studies than upon the contents of his Heidelberg lectures on anthropology. The new course seems also to have owed something to Wundt's recent occupation with Aristotle.

During his whole long stay at Heidelberg Wundt was busied about his titular subject of physiology. But he also became variously interested in philosophy, and as early as 1866 published his first philosophical book, *Die physikalischen Axiome*. Towards the last this interest must have been recognized as dominant by his colleagues; Kuno Fischer told him later that he had intended to recommend him to a philosophical chair in Heidelberg. The call came, in fact, from Zurich: at first, apparently after the publication of the first half of the *Physio-*

*logische Psychologie*, as a tentative enquiry, and a year later in definitive form,—a call to the chair of inductive philosophy vacated by Lange. Wundt was not fated, however, to remain long abroad. In May, 1875, he was offered a chair of philosophy at Leipzig. The faculty had first called Fischer, who was unable to accept. Then the idea arose, partly in view of the great age of Drobisch, of dividing Ahrens' professorship, and of securing at the cost of a single philosopher of repute two young men, the one of whom should represent the philological, the other the scientific aspect of academic philosophy. The choice fell upon Heinze and Wundt, whose youth and insignificance inspired a satirical journalist of the day to transform their names into the familiar Hinz and Kunz. One wonders how long that journalist lived! The moving spirit in the whole affair was, curiously, the astrophysicist Zöllner, a man known to psychologists both by his 'illusion' and by his unfortunate association with Slade. Zöllner had a strong leaning toward philosophy, concerned himself actively with the filling of the chairs, and was allowed by an apathetic faculty to pick his men. The Herbartians, Drobisch and Strümpell, had nothing whatever to do with Wundt's appointment,—and another historical anecdote turns out to be mere invention.

Wundt's arrival at Leipzig brings us near the end of his story. He was privileged there to enjoy the friendship of E. H. Weber, the father of experimental psychology, who had still three years to live, and of Fechner, the founder of psychophysics, who lived till 1887. From the first he aspired to organize and direct a laboratory. He started with a few rooms in the old *Konviktgebäude*; in 1892 he obtained more adequate quarters in the *Trierianum*; and in 1897 he moved back to the main building of the university, and took possession of the new-built Institute. In 1913 an extra story was added to the *Paulinum*, over a great part of the Institute proper, and was assigned to *Völkerpsychologie*. Other overhead construction was planned, with a view to a special section of psychophysics; but the war intervened, and psychophysics is now temporarily housed in some of the rooms intended for *Völkerpsychologie*. The three-fold Institute is therefore not yet complete, though it is all laid out and, apparently, promised; and if Wundt did not live to see his early dreams come fully true, we must remember that the scale of actual accomplishment far exceeds anything that he could have hoped for; the half has again proved to be greater than the whole.

In thus restricting myself to biography and to notes on Wundt's principal psychological books, I have left out of account what to most readers will be the really interesting parts of his last work. Beside the scientific homilies to which I have referred we have vivid sketches of men and their manners, of universities and cities; pregnant essays upon certain large questions of education; and a final section on the practical ethics of the Great War. Wundt is animated by a fervid and somewhat narrow patriotism. It is a thousand pities that he never visited England or America; the Anglo-Saxon genius is as foreign to him as it was familiar to Helmholtz, and he writes of the two countries with all the assured superficiality of the outsider who has facts but no perspective. His attitude to the future of Germany is that of chastened but convinced optimism. The *Volk* has been purged in three fires: the Reformation, the Thirty Years' War, and the Great War. It must and will, after the throes of present revolution, return to its native idealism and on that basis reconstitute the German State, which



will then play its due part in the furtherance of civilization.—But this and many other passages of *Erlebtes und Erkanntes* demand fuller consideration than is allowed by the limits, however generously drawn, of a book-review.

Fuller consideration of the book as a whole, taken together with many other books, is also needed before *der innere Zusammenhang* of Wundt's life, which his preface leaves us to make out for ourselves, can be brought into clear light. We see that he was one of those not uncommon natures that combine a large capacity for day-dreaming with an obstinate persistence in the day's work. We see that he had a steady dislike of routine imposed from without, though he presently laid a scholar's regimen upon himself; he shrank from the life of a schoolmaster or medical practitioner; he escaped to Tübingen and clung to Heidelberg that he might work and play at his own sweet will. We see that his endowment was general and not special; with better schooling, he might have become a philologist; as it was, he might have been a chemist or a professor of pathological anatomy, or might have remained a physiologist and attained to eminence in that field; and if he was not in fact a politician, he would have made a very good minister in a bureaucratic government. Apart from these generalities three things, in particular, have impressed themselves on me as I have read. The first is Wundt's quite unusual combination of the experimentalist with the generalizing logician. Few men of science, even great men, hold the balance of interest as truly as Wundt held it. The second thing is his extraordinary power of 'cram.' He could keep available an extended and intricate subject-matter for as long as he needed it, and could then drop it, and turn fresh to a new topic. I know no better word than 'cram' to use for this capacity, but it was cram carried to the nth degree. The third thing—a character that I have called attention to in my biographical paper—is Wundt's imperative tendency to systematize the unripe. He was not a systematist of pure blood; for that, his thinking (if I may say so with all respect) was too turbid, too little clear; neither, of course, was he the mere encyclopaedist; he was continually essaying system. All these points are illustrated in his sixty years of psychologizing. They do not account for the psychological trend: if we are ever to understand that, we must know more of *Angelegtes*, and relate it both to *Erlebtes* and to *Erkanntes*.

E. B. T.

*L'année psychologique*, vingt et unième année (1914-1919), ed. by H. Piéron. Paris, Masson et Cie., 1920, pp. xii, 522. Price 35 fr.

Psychology welcomes the reappearance of *L'année psychologique*. Founded in 1894 by Alfred Binet, it appeared regularly every year until the 20th volume had been published in 1914. Then the war intervened, and for the next six years publication ceased. Now it appears once more as a sign that French psychologists are again at their habitual work. The '*Année*,' as it is familiarly called, has from the first occupied an unique place among psychological periodicals. It was designed, as its title implies, principally as a year-book of psychology. It was the founder's original plan to divide the book into three parts: the first to contain original articles, essays and experimental investigations (chiefly from the laboratory of the Sorbonne), and general reviews of important topics written by experts in their special fields; the second to consist of digests of all the significant books and monographs of psychological interest which had appeared during the year; and the third to be a psychological index for the year. With this

schema it was his policy to cover as much of the whole range of psychology as possible for the twelve-month period. Both schema and policy were maintained until 1904, when the policy was broadened to include occasional articles and reviews by specialists in fields adjoining psychology,—particularly in cytology, in the anatomy, physiology, and pathology of the nervous system, in mental pathology, anthropology, criminology, the pedagogy of the normal and abnormal child, ethics, and philosophy. In order to make room for this extension of programme the psychological index was dropped. Then, in 1908, came a still more marked change in policy; for in that year Binet announced his purpose henceforth to devote the major part of the *Année* to problems in practical and social psychology. This plan was put into effect and adhered to until his death in 1911. In 1912 the *Année* was edited by Languier des Bancelles and Th. Simon, and consisted entirely of *mémoires originaux*. The following year, 1913, the editorship was assumed by H. Piéron, Binet's successor at the Sorbonne, who has in general followed the policy laid down by Binet in 1904.

The high quality of the *Année* throughout its history is beyond question. Many of the research articles, particularly those written by Binet, alone or in collaboration with Simon, have had a decided influence upon the subsequent history of psychology. Furthermore, its pages have frequently been the means of publication for the best contemporary psychologists of France, for Beaunis, Ribot, and Henri (all three of whom were at one time or another co-editors with Binet), for Bourdon, Féré, Foucault, Piéron, Janet and Wallon. But the uniqueness of the *Année* lay in the nature of its general reviews, and in the character of its *analyses bibliographiques*. The former possessed an insight and an authority which still make them valuable as summaries of the period which they reflect; the latter had as ideal so thorough an abstraction of the principal features of method and result as to make it unnecessary for the reader to revert to the original articles. It goes without saying that this aim is in many instances impossible of fulfillment; but with the frequent reprinting of tables and curves, and of figures of apparatus, the digests, made for the most part by the editorial staff, attained as a whole a quite unusual completeness and reliability.

The present volume is a monument to the industry and enthusiasm of Piéron. He and his students have contributed three of the experimental studies, and he himself has written two of the reviews, nearly all of the 600 *analyses bibliographiques*, and the *chronique*; and as a result he, with the aid of his collaborators, has succeeded in producing a volume that is a worthy representative of the series. The first of the *mémoires originaux* is an experimental study of expectation (*problème de l'attente*) from the Sorbonne laboratory by Mlle. Morand; there follow experimental investigations of auditory space perception (*Recherches sur les perceptions spatiales auditives*) by B. Bourdon, of the persistence of an acquired ability (*La persistance des aptitudes acquises*) by M. Foucault, and of a comparison of the memory for meaningless forms with that for numbers (*Recherches comparatives sur la mémoire des formes et celle des chiffres*) by H. Piéron. Following these is an essay on the psychoneuroses (*Essai psychologique sur les psychonévroses*) by M. Mignard and A. Gilles; then comes another experimental study from the Sorbonne laboratory on the cor-

relation of apprehension with memory (*Capacité d'apprehension; rapidité d'acquisition et puissance de rétention de souvenirs bruts: Recherches de corrélation*) by Ot. Vlaïcou; and finally an essay on the types of aesthetic ideas (*Les types d'idéation esthétique*) by M. Grzegorzewska. One cannot escape the impression that, with the exception of the essay on the psychoneuroses, and the investigations of Foucault and Vlaïcou which are perhaps still typical of research in educational psychology, these studies are a little out of tune with present-day psychology. Morand, for example, is content to rest the case of expectation with the conclusion that it is a conscious attitude of the intellectual sort; Bourdon does not bring his results into line with recent experiments on the localization of sound; and Piéron would hardly conceive of his problem today as he did in 1912 when his work began. It is only fair to say that, but for the war, the list would probably have appeared in 1915. There is, however, a result of Bourdon's that is more opportune. He concludes that, aside from intensity, the principal cue to the localization of sound both in distance and in direction is a character which he calls degree of *netteté*. He does not think that timbre in its own right can have much influence on localization. The degree of *netteté* is conditioned in part upon the distance of the sound, in part upon the position of the head relatively to the source of sound, and in part upon differences in timbre (the telephone click is more *net* than the sound of a whistle, and the whistle than the sound of a tuning fork). The psychological nature of *netteté* is, however, not made explicit; other descriptive expressions are clear, clean (*net*), pure (*franc*), distinct (*clair*); "the sound of a whistle," Bourdon says, "seemed to me less soft, less muffled (*moins flou, moins étouffé*) when it was near than when it was far." Although the general significance of these terms is obvious, it is also clear that further analysis is necessary. Whether Bourdon has discovered a phenomenological basis for the localization of sound more study alone can tell; but it would seem that he has shown a way to experimentation that should clear up a long-standing problem.

The Notes and Reviews of this volume of the *Année* consist of a short paper on the *correspondence of intelligence with instinct*, by E. Rabaud; a review of more than 100 titles on the *psychoneuroses of the war*, by H. Wallon; an account of the work done during the war by England, France, Italy and the United States on the examination of aviators (with a bibliography of 67 titles) by H. Piéron; and a report, also by Piéron, of the meeting of the newly-formed psychological section of the French association for the advancement of science held at Havre, July 27-29, 1914. The *analyses bibliographiques* represent a brave attempt to report the principal studies in psychology and neighboring fields during the years of the war. But they are limited principally to American, British, French, Italian, and Swiss sources; with the exception of 7 titles from Pflüger's *Archiv*, no Austrian, Danish, German, or Swedish papers or books are mentioned. The principal parts of the *Chronique* are a note on the part played by psychology in the war; the necrology for the six years (given, unfortunately, without dates); the events of the period, particularly as regards the vicissitudes of psychological periodicals during the war; and finally a refutation of the statement made by an American psychologist that psychological publications in the French language are, in the light of a statistical study, on the decrease. With an author's index the volume is brought to an end.

H. P. W.

*Vom Organismus der Sprache und von der Sprache des Dichters. Zur Systematik der Sprachprobleme.* By MARGARETE HAMBURGER. Leipzig, Felix Meiner. 1920. pp. vii, 186.

The author finds such days as these well spent in taking measure of the cultural attainments of one's country. Her own contribution is to be a study of speech, since a people's speech parallels its intellectual development; and her particular concern here is the aesthetic side. But two hundred words give small space in which to do justice to one's mother tongue in all its aesthetic aspects, especially when allowance must be made for the repetition and overlapping of a style which is not succinct and an organization by no means compact. And the author's range is ambitious; theories of the origin of speech, the history of its development, an estimate of it as art and as a medium of art, a formal criticism of poets' language are her main headings; and she touches upon other problems, as the possibilities of translation, the existence of synonyms, the nature of genius. Consequently the theories are not always given the evaluation which makes such summaries most worth while; problems are dismissed with the lining-up of authorities, or with a mere statement of opinion; and generalizations are couched in terms which must lead some readers to hope that they were written more easily than they are grasped.

However, the point of view is clear enough,—and incidental in its exposition are pleasing passages of literary criticism. Speech in its beginnings is more than a vehicle of information: it is always a sign that men are not wholly absorbed in satisfying practical wants. And it is, too, at that level, more than the mere play of imitating nature's sounds. The artistic impulses which make poetry are at work when the speech of a people is formed; the early speech is poetry. In it, and even here and there in its descendant, our own talk of every day, we may expect to find the very stuff which we are accustomed to attribute to the poet's peculiar gift: rhythm and rhyme, and the figures of speech. Metaphor, indeed always takes a large part in the making of a language. It intensifies the impressions of one sense by bringing to them those of another. It names new ideational experiences which have no counterpart in expression. And here it is in the nature of metaphor that it conveys the meanings in imaginal form, and keeps always in the hearer the sense of the image. But its intention never is to illustrate; its fitness consists in the unity of its effect: its various images do not become explicit and discrete, but rather give richly on the side of mood. "Grau, teurer Freund, ist alle Theorie, und grün des Lebens goldner Baum" is the example quoted from a later, individual maker of language.

But speech must grow along with the thought of the race. The demand which German thought in its development made of language was that it became less palpable and more abstract. The music was lost, and the metaphors faded to symbols. There is, of course, gain of a sort, and one which the author does not underrate, for the speech which carries evolved thought: it must become highly organized and variously shaded. But it comes a long way from poetry; and the poet meets a difficulty, which gives other artists little worry, in this medium which has lost so much of its vividness in serving non-artistic ends. In his remolding of it for his use, however, it comes to a new birth; and we are to consider that here the making of speech, which has been the work of the race, passes into the hands of creative individuals. Even the smaller men among them have a share in the achievement, for they help in restoring and putting into circulation the

old sensory meanings of words, and whatever the centuries may have given in the way of emotional accruals. Or in depicting situations for which current German is especially inadequate, the dialects with their rhythms and moods may be adapted. But revival is not sufficient, and at various times, in protest against the flatness of common speech or heavy tradition of literature, new language has been made. Such a spirit, with the need for a new form to match their thought, inspired the *Stürmer und Dränger*.

This is the outline of Hamburger's view. One of the questions to which it brings her, naturally, is the degree to which speech created by the poet may influence, and come into, the speech of the people. Authorities say that the very color of Goethe's language made it less usable by others; words and structure might be paralleled, but the values were his alone. Other authorities say that Goethe gave German new vigor and new dimensions which are more precious than any material enriching through new colors and tones. The author herself brings instances of words which are the gifts of artists and philosophers; and concludes in general terms that, in so far as the poet crosses the limits of his individuality to find his highest development in the whole, there is harmony between his speech and the speech of all.

Vassar College

J. GLEASON

*Absählende Methoden und ihre Verwendung in der psychologischen Statistik.* By O. LIPMANN. Leipzig, J. A. Barth, 1921. pp. 78.

This monograph, as its title suggests, deals with the statistical treatment of mass-results rather than with the older established psychophysical methods. The view taken is from the standpoint of the individual within a group of performances, or of the group within different performances, or of different groups within the same performance. The treatment is almost wholly graphical or tabular in nature; very little importance attaches to any single representative value except as it serves as basis for the determination of some new curve. The idea of units of measurement is implied in the taking of the data, in the computation of the single values, and in the numerous interpolations; the actual comparisons are between curves or parts of curves. We have the empirically determined curve given as the standard of reference; hence the measure of any performance is a relative and not an absolute matter. Interest, therefore, seems to attach to differences rather than to uniformities; to the results as a whole rather than to an abstracted result; to relative rather than to absolute values.

The methods and practices are those of the natural sciences and of education,—of the biologist, the mental tester, and the worker in the field of experimental education. The psychophysicist will find the monograph interesting inasmuch as it presents a mode of treatment (it treats of psychophysical problems) which differs from that of the regular psychophysical methods; and he may be brought to believe that a relative judgment, after all, is as far as we may safely go in view of the present state of our knowledge about absolute units.

Because of the complexity and the unobservable nature of the conditions which obtain in psychological work, the mode of statistical treatment given to psychology by the natural sciences is inadequate to psychological data; variations and modifications must be made. The author emphasises the importance of these reforms. No less than fifteen varieties of curves are worked out and graphically illustrated, and more than a dozen others are suggested on the analogy of those



given. Wealth in abundance! so one, at first, inclines to say; why, however, do we need a round two dozen measures for one and the same thing? To be sure, every curve or set of curves shows a somewhat different relation between the sets of data, or shows the relation in a somewhat different way.

The more fundamental forms of curve are: the curve of actual distribution, the curve of difference in actual distribution, the curve of percentage distribution, the curve of difference in percentage distribution, the curve of rank order, the curve of difference of rank order, the curve of rank order according to percentage, the curve of difference in rank order according to percentage, the zonal curve in which the point on the abscissa which corresponds to the value found for the measure of central tendency taken has zero-value in terms of the ordinate scale, the zonal curve for percentages, and, finally, curves of distribution of absolute or rank differences of scores in the total series or rank orders of these differences. It is not to be supposed that all these curves employ the crude data, or that the crude data belong exclusively to any curve or set of curves. The notion of the quartile finds extensive application. Since the significance of the difference between the upper and lower quartiles depends in part upon its relative magnitude, measures of variation take the quartile values in relation to some optimal value.

The longest section in the monograph deals with the subjects of correlation, coordination and contingency. As is to be expected, the curves of regression receive more emphasis than does the bare coefficient of correlation. Coordination resolves itself ultimately into equations expressed in terms of the quartile division. The test for contingency receives but little consideration, though it is perhaps adequate for the worker in the field. The monograph closes with a brief discussion of the problem of weighting results.

L. B. HOISINGTON

*The Labor Movement: Its Conservative Functions and Social Consequences.* By FRANK TANNENBAUM. New York, G. P. Putnam's Sons, 1921. pp. xviii, 259.

Only one chapter in this book falls by title within the province of the JOURNAL, that on "Labor Movement Psychology." The remaining chapters, with their doctrines of the overthrow of the "profit motive" in business, the "displacement of the capitalist system by industrial democracy" and the "rebuilding" of our "social structure" by the labor movement, the development of new industrial "political institutions" as the "inevitable" outcome of the worker's activity, all deal with matters which challenge the professional attention of others than the psychologist. If a reply is to be made to the author's persuasive prophecy that the machine will force us all to become workers, that the foes of the workers will force us all to become members of the union, and that all government will ultimately and rapidly become a form of glorified labor congress with the workers as congressmen, it will be better made by specialists in government.

In the chapter on Labor Movement Psychology, the worker is described as "propertyless," "a wanderer," "a seeker for better things" "a cog." In his work his "self and personality are not involved." His employment is "monotonous," "unbearable." He has neither the "satisfaction of a job well done" nor the joy of completing a "self-made plan." For him the present is "accidental, transitional," the future hopeful. His "drifting body tends to carry with

it a restless mind," a "mind that knows nothing of the conserving, constructive experience" of those who are stabilized, for instance, by owning their own land. "Unemployment," "industrial irregularity," "occupational accidents," "the competitive and shifting market," the "adventure of ordinary business," the advent of the machine, have all resulted in "agitation of the mind and discomfort of the body" for the worker. The industrial revolution has equalized the "imagination" of men, and has increased their "irregularity of possession." Now the poor feel "spiritually" equal to the rich. The worker wants much but gets little, and is consequently dissatisfied. Equality in all but money makes money conspicuous, and so simple a difference between the poor and the rich makes the difference "more objectionable," and "desire for adjustment more vivid." Whatever unhappiness the worker feels is the stronger because it is constant and unescapable. He is driven, and driving "suppresses personality," and suppression of personality leads to "rebellion." The intellectual and critical nature of the labor movement in itself makes trouble. "Thoughtful workers" believe the world's ills could be cured "if only there were the will and the desire" to remedy them. "The strength of this conviction is in proportion to the revolutionary idealism of the workers. This conviction leads to the conclusion that the present system is not only bad but is kept so by the perfidy and selfishness of the powers who are benefiting by the present system. It thus adds to the hatred and to the instinctive opposition against being reduced to mechanical instruments by the machine, the belief in the villainous character of the capitalists as a class, a conviction that adds contempt to hatred and leaves a constant bitterness that knows no end." "Opposition by the capitalists—an opposition that is based often upon ignorance and generally upon selfishness and class standards—makes for a constant aggravation of this bitterness." His "psychological maladjustment" makes the worker a ready prey to "emotional appeals" which offer "an easy and ready rationalization of the world and its implications." "The labor movement provides an emotional outlet. It provides room for creative activity." But it does all this "in terms of the values, functions, and problems with which the worker is called upon to deal."

Obviously, such a chapter is not meant as a contribution to psychology. The author simply passes in review the events of the worker's day and the ideas which occupy him, and records the emotional value which he deems them to possess. Psychology has come to mean merely a plausible explanation of everyday experience. It is but fair to repeat that the emphasis of the book falls elsewhere.

Cornell University

H. G. BISHOP

*The Origin of Man.* By CARVETH READ. Cambridge, University Press, 1920. Pp. xii, 350.

This book may be characterized as a psychological interpretation of the better known facts concerning Magic, Animism, Totemism, and concerning the priests and wizards who practice or encourage those rites and beliefs. The first two chapters trace the development of man from an hypothetical primitive stock, a cross between the ape on the physical side and the wolf on the mental, which the writer calls the lycanthropoid stock. These results are highly speculative, obviously. The chapter on Belief and Superstition is an interesting application of the psychology of reasoning, with illustrations from many sources. Much discussion is directed to the problem of the priority

of Magic or Animism, with the conclusion that Magic probably comes first; but the interactions of the one upon the other are also traced through two chapters.

The long discussion of the origin of totems inclines to accept Andrew Lang's view that they were names given by outsiders to a group and gradually accepted by the members of the group themselves. Later savage explanations gave rise to many myths which vary from tribe to tribe. The writer believes that exogamy and other marriage customs grew up independently because of social needs and by natural selection, and that the connection established between them and the totems is more or less adventitious. A final interesting chapter traces the historical relations between magic and science, with the conclusion that magic did rather more to hinder than to advance the cause of science.

The book contains little new material, nor all of the very recent material, but is an important working over of anthropological results for the light they cast upon, and receive from, certain psychological doctrines. On many points it may well be recommended to the student of psychology.

*University of Michigan*

W. B. PILLSBURY

*The Psychology of Thought and Feeling.* By CHARLES PLATT. New York, Dodd, Mead & Co.

This is avowedly a popular presentation of psychology from the point of view of a modified Freudian. It tends to become a discussion of things in general, with a few psychological principles serving as texts. As one might expect, instincts, emotions, and the unconscious form the body of the book. The chapters on Memory, Reason, and Habit contain pleasant descriptions of the superficial processes, with no reference to experimentally determined laws, nor attempt at fundamental analysis.

The last five chapters deal with applications: Education, Mental Ills, The Crowd, The Delinquent. In these, as in less degree in the earlier part of the book, the author appeals to the Unconscious to solve all difficulties. It is a *deus ex machina* which itself needs no explanation. The book is one of the best warnings so far of what we may expect if that tendency should become dominant in psychology. One may quote the writer's own dictum: "'Overbeliefs,' those which are held without the foundation of fact, are no more true when they come through the gateway of science than when they lack this prestige."

The book should appeal to the general public, as it is well written, with many interesting illustrations.

*University of Michigan*

W. B. PILLSBURY

## PSYCHOLOGICAL PERIODICALS

*Zeits. f. Psychologie.* Bd. lxxxiv., Heft. 1-3. P. BUSSE. 'Ueber die Gedächtnisstufen und ihre Beziehung zum Aufbau der Wahrnehmungswelt.' [Reports experiments, performed in Jaensch's laboratory, on three types of image chosen from the memory-continuum (after-images, images of sense-memory, memory-images proper), with observers 11 to 17 years of age. Laws of the comparative behavior of these images are made out for change of the spatial relations of subject and object, for range of concomitant field of view, for clearness, for power of suppression and resistance; and the influence of sense-memory upon perception is demonstrated.] W. FUCHS. 'Untersuchungen über das Sehen der Hemianopiker und Hemiambyopiker: i. Verlagerungserscheinungen.' [Third of the series of papers edited by Gelb and Goldstein. Errors of localisation, both relative (tendency toward the point of fixation) and absolute (shift of visual space at large), are explained in terms of attention, which is however taken not as mental faculty but as 'structurally determined reaction' in Köhler's sense.] Literaturbericht.

Bd. lxxxiv., Heft 4-6. A. GELB. 'Ueber den Wegfall der Wahrnehmung von "Oberflächenfarben:" Beiträge zur Farbenpsychologie auf Grund von Untersuchungen an Fällen mit erworbenen, durch zerebrale Läsionen bedingten Farbensinnstörungen.' [Fourth paper of the Gelb-Goldstein series. Reports cases in which there was loss of surface colors (Katz); and all visual objects appeared filmy or filmy-bulky. Persistence of object-consciousness assured the color-constancy of these films.] H. FRIEDLAENDER. 'Ueber Gewichtstäuschungen.' [Reports experiments on the resting skin. Illusions with stimuli differing in material are due to expectation; those with stimuli differing in volume to expectation *plus* the influence of 'seen' density. The 'idea of expectation' involves a complex play of the unconscious associative mechanism, and its effect may be a positive as well as a negative (contrast) suggestion.] K. SCHOLL. 'Vom absoluten Eindrucke bei Schallstärkevergleichen.' [Attempts to prove the relative (empirical) nature of 'absolute impression' by the effect of series of single (loud or weak) or paired sounds interpolated between regular series. No clear result can be made out.] Literaturbericht.

Bd. lxxxv., Heft 1-4. Festschrift zum 70. Geburtstage von Prof. Dr. Georg E. Müller. J. FROEBES. 'Aus der Vorgeschichte der psychologischen Optik.' [Outlines the views of the Greek, Alexandrian and mediaeval periods (Aristotle; Euclid; Alhazen, Vitello, R. Bacon).] E. R. JAENSCH. 'Zur Methodik experimenteller Untersuchungen an optischen Anschauungsbildern: z. T. nach gemeinsam mit F. Reich durchgeführten Versuchen.' [The images of sense-memory follow the general laws of perception: identity of visual direction, retinal incongruity. The eidetic *Anlage* is found in two types of constitution, the tetanic and the Basedow; most cases are clinically quite normal.] D. KATZ. 'Psychologische Versuche mit Amputierten.' [Summarises observations on the illusion of the phantom limb, on the sensitivity of the stump, and on the behavior of muscles after Sauerbruch's opera-

tion. The perception of weight arises, not in the joints, but in muscle and tendon.] O. KRON. 'Eidetiker unter deutschen Dichtern: ein Beitrag zum Problem des dichterischen Schaffens.' [Discusses the images of sense-memory in the cases of O. Ludwig, L. Tieck, E. T. A. Hoffman, J. V. von Scheffel, and Goethe.] G. RÉVÉSZ. 'Prüfung der Musikalität.' [Describes and evaluates tests of rhythmical sense, absolute and relative ear, recognition of octaves, analysis of dyads, triads, etc., the catching and repeating of melodies, playing by ear.] E. RUBIN. 'Vorteile der Zweckbetrachtung für die Erkenntnis.' [A teleologically directed *Aufgabe* has the advantages of cumulative pleasure, analytic procedure, results from unsystematic work, availability of organised memory-material.] F. SCHUMANN. 'Die Repräsentation des leeren Raumes im Bewusstsein: eine neue Empfindung.' [Reduces vision of empty space (as in stereoscopic combinations) to a true sensation, the 'glassy' sensation.] W. BAADE. 'Zur Lehre von den psychischen Eigenschaften.' [Psychology has two parts, which deal respectively with the phenomena of consciousness and with the psychical attributes of the organism. These attributes are in turn either general, whose adaptive reactions bear witness to a principle of continuity, or circumscribed, whose functions are approximately stereotyped. The writer advocates a special dynamology, or science of the circumscribed attributes (Beneke's *Angelegenheiten*, Wundt's psychical dispositions, Müller's residues).]

Bd. lxxxv., Heft 5, 6. L. BOUMAN und A. A. GRUENBAUM. 'Kasistischer Beitrag zur Vorstellungspsychologie.' [The self-centred day-dreams of a patient who, under laboratory tests, shows small power of visualisation are made up of vivid visual, auditory, haptical images. There are thus objective and personal types of ideation.] J. PLASSMANN. 'Säkulare Veränderlichkeit des Dezimalfehlers.' [Continues records from 1917 to 1919.] H. KELLER. 'Eine Verbesserung am Hipschen Chronoscop.' [Rotary dial.] Literaturbericht.

Bd. lxxxvi., Heft 1-3. W. FUCHS. 'Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker: ii. Die totalisierende Gestaltauffassung.' [Fifth paper of the Gelb-Goldstein series. Experiments show that, under certain conditions, central supplementation may give rise to the appearance of clear vision by the defective or injured parts of the retina. The conditions prove that the result cannot be explained by images or residua or attention; there is a 'totalising apprehension,' a touching-off of a central *Gesamtgestaltprozess*. This same process is normally at work in the filling-out of the blind spot, in twilight vision by the fovea, in certain observations of visual forms (Schumann, Zigler).] H. HENNING. 'Ein optisches Hintereinander und Ineinander: Gemischte Farbenempfindungen.' [(1) A simple stereoscopic arrangement permits the seeing of two colored areas at different distances with corresponding (identical) retinal areas; the nearer color is a transparent surface. (2) A modification of von Karpinska's apparatus permits the seeing of differently colored lines (under certain circumstances, areas), with corresponding retinal areas, at identically the same place (*am gleichen Orte der Kernfläche*); the result depends upon accurate fusion of contours and energy of attention. (3) A *Nachtrag* discusses related phenomena and physiological conditions.] Literaturbericht. Gesellschaft für experimentelle Psychologie. [Notice of 7th Congress.]



Bd. lxxxvi., Heft 4. E. KALLA. 'Eine neue Theorie des Aubert-Försterschen Phänomens.' [Haploscopic experiments show that the Aubert-Förster phenomenon (optical figures that are small and near are recognized over a larger portion of the field of vision than large and distant figures appearing under the same visual angle) is independent of accommodation and convergence; Jaensch's theory is thus refuted. In point of fact, distant objects arouse more residua than near, and this arousal means mutual inhibition, which is therefore the key to the phenomenon. The Koster phenomenon (enhanced *Eindringlichkeit* in micropsia, and under conditions which destroy visual objectivity) is to be explained similarly, as due to the cutting-off of residua.] K. WINZEN. 'Die Abhängigkeit der paarweisen Assoziation von der Stellung des besser haftenden Gliedes.' [Varied experiments prove that, if two ideas are to be associated, and the one is more holdfast (*besser haftend*) than the other,—more familiar, more insistent,—it is of advantage for retention that the more holdfast idea come first and not second. Hence in learning vocabularies one should place the foreign word to the right, not (as is usual) to the left of the native word.] W. HELLPACH. 'Zu der Bemerkung des Herrn Brugmans in Heft 5/6 des 85. Bandes dieser Zeitschrift.'

Bd. lxxxvi., Heft 5, 6. F. SCHUMANN. 'Die Dimensionen des Raumes.' [Second paper of the series *Untersuchungen über die psychologischen Grundprobleme der Tiefenwahrnehmung*, ed. by Schumann.—Raises the question whether the truly plastic spatial impression, as distinguished from that of a pictorial representation of space, is in fact tridimensional. Recent experiments have made it certain that two colors may appear at different distances in the same visual direction; and this fact, together with the existence of the colorless glassy sensation, seem to assure tridimensionality. The conclusion, however, is so far only tentative.] E. R. JAENSCH und F. REICH. 'Ueber die Lokalisation im Sehraum.' [Second paper in the series *Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter*, ed. by Jaensch.—(1) The localisation of the images of sense-memory (*Anschauungsbilder*), whether with steady fixation or with shift of the point of regard, is determined by the region of attention (*Aufmerksamkeitsort*). (2) All the phenomena observable with the use of the three actual threads of the Hering-Hillebrand fundamental experiment on horopter deviation are also observable in the parallel experiment with images of sense-memory; the same three types of observer appear, and in general an observer retains his type in passing from the one to the other experiment. If fixation is long maintained, the phenomena of the fundamental experiment are typically complicated by the supervention of images. These results suggest the use of the image as an instrument of analysis of the fundamental experiment; and it is found that if, in the observation of two or three imaginal threads in the image of sense-memory, one or two are distinguished by the attention, then, as the attention is voluntarily shifted, the preferred thread or threads shift also in the same direction. Since a near-position of attention touches off a tendency to adjustment of attention for distance, and conversely, the whole group of phenomena falls under Hering's principle of the self-regulation of living substance (cf. Mach's antagonistic processes of attention). (3) Hillebrand's explanation of the horopter deviation is unsatisfactory because his experimental arrangements were only partial. Space-values are not stable. The cue to explanation lies in the gross phenomena of the

parallel experiment; and the passage from eidetic (imaginal) through hemieidetic to rudimentarily eidetic endowment furnishes the needed continuity between the parallel and the fundamental experiments.] A. HOEFLER. 'Meinongs Psychologie.' [An appreciation of the ten papers in the psychological volume of Meinong's *Gesammelte Abhandlungen*.] Literaturbericht.

Bd. lxxxvii., Heft 1, 2. E. BECHER. 'W. Köhlers physikalische Theorie der physiologischen Vorgänge, die der Gestaltwahrnehmung zugrunde liegen.' [Popular paraphrase of the argument of Köhler's book "Die physischen Gestalten in Ruhe und im stationären Zustand" (1920), with comment interspersed in smaller type: a useful paper.] G. MARZYNSKI. 'Studien zur zentralen Transformation der Farben. [(1) A given grey in a given illumination may be objectively lightened or darkened *either* by addition of white or black *or* by increase or decrease of total or partial illumination; the resulting difference of intensity of light may then be the same, but phenomenologically the change of brightness is not identical with the change due to total or partial 'transformation.' An examination of the experimental studies of Weber's Law shows that constancy of the differential sensitivity appears only where transformation is in play. (2) A shadowed and an unshadowed paper may be compared for objective, subjective and reductive equality. The shadowed paper looks darker under the subjective than under the objective attitude, but still appears less dark than the reductive procedure makes it. Schools of painting show characteristic differences in their treatment of shadows.] E. GORRHEIL. 'Ueber das latente Sinnengedächtnis der Jugendlichen und seine Aufdeckung.' [Second paper of the series Ueber die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens, ed. by Jaensch.—Tests made by way of after-image and memory-image bring out rudiments of images of sense-memory even in cases where a direct test fails. An eidetic phase of development is therefore in all probability normal.] E. R. und W. JAENSCH. 'Ueber die Verbreitung der eidetischen Anlage im Jugendalter.' [Third paper of the series.—A statistical enquiry confirms the results of the preceding study.] A. GOESSER. 'Ueber die Gründe des verschiedenen Verhaltens der einzelnen Gedächtnisstufen.' [Fourth paper of the series.—Experiments on after-images, images of sense-memory, and memory-images, as regards mode of appearance of background, freedom from background, influence of a rotating or uneven background and of a colored projection-surface, and assimilation to objects of perception, all alike indicate that as the memory-series proceeds the connection of the image with the objects of perception simultaneously presented becomes weaker.]

Bd. lxxxvii., Heft 3, 4. B. HERWIG. 'Ueber den inneren Farbensinn der Jugendlichen und seine Beziehung zu den allgemeinen Fragen der Lichtsinns.' [This paper, the fifth of the series Ueber Grundfragen der Farbenpsychologie ed. by Jaensch, opens with a full discussion of the nature and distribution of the image of sense-memory, its differences from the ordinary after-image, the conditions of its appearance as positive or negative, etc. The author then shows that many sense-phenomena appear in the image in exaggerated and therefore more readily accessible form. The study of these imaginal effects is therefore instructive for the general psychology of vision: they may help us, *e.g.*, to distinguish color-weakness from color-blindness, inner from

outer color-blindness, etc. The phenomena of induction receive special attention, and the normal color-blindness of the peripheral retina is referred to induction rather than to anatomical insufficiency.] E. R. JAENSCH. 'Ueber Kontrast im optischen Anschauungsbild.' [Sixth paper of the series.—Contrast-effects are enhanced in the image.] B. HERWIG und E. R. JAENSCH. 'Ueber Mischung von objektiv dargebotenen Farben mit Farben des Anschauungsbildes.' [Seventh paper.—Phenomena of mixture, less often of rivalry, occur. The results of mixture confirm the status of the image.] Literaturbericht.

*Arch. f. d. ges. Psychologie.* Bd. xxxix., Heft 3, 4. B. PAULSEN. 'Einfache Reaktionen bei Variation und rhythmischer Gliederung der Vorperiode.' [The effect of equally spaced auditory stimuli in the foreperiod of auditory reaction differs with the length of the intervals and the tendency of the reactor to rhythmisation. The best result (unity of voluntary process, correct reaction) is obtained with two optimally spaced signals which are apprehended with the stimulus proper as forming an anapaest.] J. O. VERTZ. 'Das Gedächtnis der Blinden.' [The immediate verbal memory of blind children is better, both in range and in time of reproduction, than that of normal children.] J. K. von HOESSLIN. 'Die Melodie als gestaltender Ausdruck seelischen Lebens.' [Working from the definitions of melody offered by Lipps and Siebeck, the writer finds that tones affect the inner life of mind both by their manifold interrelations and by the play of tensions which they touch off. The tensions are enough: we have space-melodies in the Gothic cathedral and thought-melodies in the poems of Goethe and Lenau.] E. STERN. 'Zur Frage der "logischen" Wertung.' [Experiments on touch, taste and smell (right-wrong, present-absent) confirm Haering's thesis that cognition is an evaluation.] W. WIRTH. 'Beiträge zur psychophysischen Anthropologie. I. Anomalien der Gesichtsfarbe als Begleiterscheinungen der Farbenblindheit.' [Deuteranopes seem to be markedly ruddy, protanopes pale.]

Bd. xl., Heft 1, 2. C. THEODORIDIS. 'Sexuelles Fühlen und Werten.' [The author starts from the thesis that moral ideas and social organisation have their root in sexual evaluation, and that universally and originally the sexual act is regarded as something mysterious, forbidden, impure: this is the view of the man, who considers that he injures or defiles the woman. After adducing evidence for his views, the author considers certain problems of social psychology, connected with the beginnings of society and with the growth of law.] N. АСН. 'Zur Psychologie der Amputierten: ein Beitrag zur praktischen Psychologie.' [Discusses the mental state of the patient, and his psychological treatment; the choice and use of the prosthesis; and the education of the will to work.]

Bd. xl., Heft 3, 4. W. WIRTH. 'Unserem grossen Lehrer Wilhelm Wundt in unauslöschlicher Dankbarkeit zum Gedächtnis!' [Memorial article with two portraits.] O. KLEMM. 'Untersuchungen über die Lokalisation von Schalleizen: 4. Ueber den Einfluss des binauralen Zeitunterschiedes auf die Lokalisation.' [Discusses the binaural temporal limen, the appearance of a simple subjective field of hearing, localisation and apparent movement in the sense of the temporal difference. Experiments with the Helmholtz pendulum give extraordinarily small times: thus localisation is possible for one observer with a temporal difference of 0.002σ.] E. SCHERER. 'Das Problem der anschaulichen Gestaltung in der Lyrik.' [The sensory and affective

reality that is aesthetically characteristic of the best lyric poetry depends, not on the arousal of visual imagery, but upon verbal melody and rhythm: pitch, weight, volume of vowels, duration, accent of syllables. These elements, possibly reinforced by kinaesthesia, are responsible for the plastic effect.] J. E. LIPS. 'Die gleichzeitige Vergleichung Zweier Strecken mit einer dritten nach dem Augenmass: zum Drei-Reize-Problem in der Psychophysik.' [Under the experimental conditions the double judgment has a small advantage in accuracy over the corresponding single judgment. Under the same conditions the normal *DL* is approximately the same as compared with the current values of the simultaneous *DL*; new experiments, however, greatly reduce these latter.]

*Psychological Review*. Vol. xxvii., no. 1. C. E. FERREE and G. RAND. 'The Absolute Limits of Colour Sensitivity and the Effect of Intensity of Light upon the Apparent Limits.' [With stimuli of high intensity, the limits of R, B, Y coincide with those of white-light vision (the limits for G are narrower); for stimuli of equal energy at medium intensities, the limits of R, B, Y interlace. In the mid and far periphery of the retina, large differences in amount of light are needed to change perceptibly the limits of sensitivity.] H. A. CARR and M. C. HARDY. 'Some Factors in the Perception of Relative Motion: A Preliminary Experiment.' [Observations on two lights, set vertically, either one of which could be given a horizontal movement. Perceptive accuracy is increased by difference in size (or increase in combined area, or both); by inequality of brightness and decrease of combined illumination; by rate of motion; and (in general) by fixation of the stationary light. Extent of motion has no influence, and position of stationary light a variable effect.] S. D. ROBBINS. 'A New Objective Test for Verbal Imagery Types.' [Threefold presentation of specially prepared meaningless syllables. Consonants receive less attention than vowels, and short vowels much less than long. Vivid verbal imagery makes for efficiency.] 'Note on the Verbal Imagery of Stammerers and Normal Speakers.' [Stammerers pay more attention than normal speakers to consonants.] J. R. KANTOR. 'A Functional Interpretation of Human Instincts.' [We must distinguish between instinct (functioning of a connate potential reaction system) and instinctive behaviour (which comprises acquired tendencies of response). We then understand that all instincts are specific; that not a single act of an adult person is instinctive (an original response); and that the emotive situation, by dissociating reaction systems, may leave in function only some instinctive mode of behaviour.] J. P. M'GONIGAL. 'Immobility: An Enquiry into the Mechanism of the Fear Reaction.' [The expression of fear is primarily an effect of thyroxine, as that of anger is an effect of adrenin.]

*Psychological Review*. Vol. xxvii., No. 2. W. D. SCOTT. 'Changes in Some of our Conceptions and Practices of Personnel.' [Emphasises individual differences, the non-rational aspects of behavior, the definition of education as profiting by experience, the idea of workers-in-their-work as opposed to that of pegs and holes, the scientific basis of vocational guidance.] J. J. B. MORGAN. 'An Analysis of Effort.' [Effort is fundamentally a reflex response to an inimical stimulus which the normal organic response has failed to meet.] J. F. DASHIELL. 'A Comparison of Complete *vs.* Alternate methods of Learning Two Habits.' [Experiments under varied conditions on maze

running (rats, children, adults), card-sorting and adding (adults), show that learning by the complete method is the more economical procedure.] R. M. OGDEN. 'The Tonal Manifold.' [Graphic representation of the system of pitch-brightness, volume and intensity.] C. ROSENOW. 'Is Lack of Intelligence the Chief Cause of Delinquency?' [The correct conclusion from Goring's statistics is that in all probability factors other than intelligence are of greater importance as determinants of crime.]

Vol. xxvii., No. 3. C. SPEARMAN. 'Manifold Sub-Theories of "The Two Factors."' [Expands reply to Thomson in *Brit. J. Psych.*, 1916. The fundamental theory has now been demonstrated with finality.] G. H. THOMSON. 'General vs. Group Factors in Mental Activities.' [The proof of the Theory of Two Factors by hierarchical order falls to the ground, and Spearman's mathematical argument is finally proved invalid. A Sampling Theory of Ability, which considers any performance as carried out by a sample of group factors, is offered as alternative.] J. R. KANTOR. 'Suggestions toward a Scientific Interpretation of Perception.' [Perception is the conscious behavior through which are developed the meanings of objects and relations which operate in the adaptation of the individual to his surroundings and in their control.] E. C. TOLMAN. 'Instinct and Purpose.' [Instincts are defined as (1) determining adjustments, often hierarchically arranged, which set in readiness particular groups of (2) variable subordinate acts. Purpose is the interaction of (1) and (2).] S. B. RUSSELL. 'Brain Mechanisms and Mental Images' [Mental images occur only when there is coordinated molecular response in a brain centre conditioned by a former environment. The response is conditioned by means of registering mechanisms in the centre and association mechanisms.]

Vol. xxvii., No. 4. W. S. HUNTER. 'The Modification of Instinct from the Standpoint of Social Psychology.' [The social significance of instinct depends largely on the modification (structural, temporal, adaptive) that instinctive forms of behavior undergo under the influence of intelligent behavior.] E. ISAACS. 'The Nature of the Rhythm Experience.' [Rhythm is the experience arising from the periodic, pendular, reflex response of characteristic organs to objective stimulation. It thus involves perception of stimuli, experience of periodic reflex response, accentuation and grouping by attention, and feeling due to repeated movement.] G. H. THOMSON. 'A New Point of View in the Interpretation of Threshold Measurements in Psychophysics.' [Emphasises the influence of 'moral' characteristics or 'mood' upon the *DL*.] J. W. BRIDGES and V. M. DOLLINGER. 'The Correlation between Interests and Abilities in College Courses.' [On the face of the returns relative interests are an extraordinarily inaccurate symptom of relative capacities; but the problem is highly complex.] R. H. WHEELER. 'Visual Phenomena in the Dreams of a Blind Subject.' [After sixteen years of blindness the dreams show three peculiarities; the persistence of the synaesthesia of the waking life, the reduction of voices to the subject's own vocal-motor imagery and their differentiation by color, and a tendency to visualise himself at a distance.]

Vol. xxvii. No. 5. L. T. TROLAND. 'The Physical Basis of Nerve Functions.' [Discusses, on the basis of the work of Nernst, Lillie and Lucas, the general mechanisms of excitation and stimulation, the specific mechanisms of the threshold, impulse propagation, the ener-



getics of nerve processes, the all-or-none principle, and the mechanisms of synapse and receptor; and makes psychophysiological applications.] R. H. WHEELER. 'Theories of the Will and Kinaesthetic Sensations.' [The historical divergence of opinions is due to the prejudice in favor of an unique mental process, to systematic bias and to lack of introspective data. The 'unique mental process' in will is simply kinaesthesia.] W. R. MILES. 'A Pursuit Pendulum.' [The instrument enables us to measure quickness, precision, steadiness of movement in reference to a moving object.] C. E. FERREE and G. RAND. 'The Limits of Color Sensitivity: Effect of Brightness of Preexposure and Surrounding Field.' [Color zones are widest when preexposure and surrounding fields have the brightness of the color. Difference of surrounding field may narrow a zone by 11°; of preexposure field, by 17°; of both together, by 20°.]

Vol. xxvii., No. 6. A. S. OTIS. 'Do We Think in Words?' [Thought is restricted neither to the material of language nor to the action of the language mechanisms.] C. H. WOOLBERT. 'A Behavioristic Account of Sleep.' [Sleep is a matter of the efficient domination of the upper muscular systems by the lower, operating through the relaxing power of fatigue.] E. S. ROBINSON. 'The Compensatory Function of Make-Believe Play.' [Play, the more private forms of fantasy, much lying and story-telling, and the appreciation of stories all serve the same fundamental purpose as compensatory mechanisms.] E. G. BORING. 'The Control of Attitude in Psychophysical Experiments.' [Gives rules, in reply to Thomson, for the regulation of attitude in psychophysical experiments.] L. A. JONES and P. REEVES. 'The Physical Measurement and Specification of Color.' [Discusses uses of spectroradiometer, spectrophotometer, colorimeter.] S. L. PRESSEY. 'Suggestions Looking toward a Fundamental Revision of Current Statistical Procedure as Applied to Tests.' ['Reliability' simply means consistency, and 'validity' does not assure value. Tests should be built specifically for concrete problems, and their value measured by their efficiency in dealing with these problems.]

Vol. xxviii., No. 1. M. W. CALKINS. 'The Truly Psychological Behaviorism.' [Neither Watson nor Warren, but only the self-psychologist, may lay claim to a genuine behavioristic psychology, a study of the totally integrated individual in the attitudes whereby it confronts its environment.] J. R. KANTOR. 'An Attempt toward a Naturalistic Description of Emotions, i.' [The distinctive mark of emotion is the absence in the act of an organized response-system; emotion is a momentary condition of 'no-response.' Analysis reveals three phases: an act of simple apprehension, disintegration of the constitutive response-systems (emotive activity proper), and superseding organic or other activities. Since emotion appears only under definite external conditions the question of inheritance is futile.] H. LUNDHOLM. 'The Affective Tone of Lines: Experimental Researches.' [Lines appear to imitate in their movement the motor expression of emotions, and therefore arouse feeling-tone.] S. C. PEPPER. 'The Law of Habituation.' [Secular changes of appreciation, 'value mutations,' are accounted for by a law of habituation. Under repeated stimulation, the members of a linked affective series swing, continuously or cyclically, from dislike to liking.] W. SCHILLING. 'The Effect of Caffeine and Acetanilid on Simple Reaction Time.' [The drugs, in 5-grain doses, retard reaction time and increase unsteadiness. Nothing positive can be said of an effect on pulse and breathing.]

## NOTES

### ON THE PLAN OF THE *PHYSIOLOGISCHE PSYCHOLOGIE*

The letter of Decr. 8th, 1872, in which Wundt offered the *Physiologische Psychologie* to the firm of Engelmann, contains an outline of the work, which shapes as follows:

- I. Physiological Properties of the Nervous System
- II. Sensation and Idea
- III. Organic Movements
- IV. Critique of Psychological Doctrines
- V. General Theory of Psychophysical Occurrence

Since II and III comprise "the empirical material of physiological psychology proper"—"the inner or psychological and the outer or physiological consequence of the manifold interactions between our outer and inner experience"—the schema may be rewritten thus:

- I. Physiology
- II. Physiological Psychology
- III. Psychology
- IV. Theory of the Relation of Inner to Outer Experience

where the *Physiology* stands for relevant, i.e., neural physiology; the *Psychology* stands mainly for the trends (Wolff and Kant, Herbart and Beneke) against which Wundt was struggling; and the concluding *Theory* discusses, in a wide sweep, "the relation obtaining in the last resort between the worlds of inner and of outer occurrence, and the explanation of the whole interconnection of psychological phenomena suggested by the survey of their borderland." The programme is clear and logical.

The published book is built, however, on a different plan. I shall first give its contents *with one omission*.

1873. Introduction, pp. 1-20.

I. Physiological Properties of the Nervous System, pp. 21-272.

[A. Inner Aspect of Physiological Psychology]

II. Sensation and Sense-Feeling, pp. 273-463.

1874. III. Idea and Aesthetic Feeling, pp. 464-706.

IV. Association of Ideas and Emotion, pp. 726-819.

[B. Outer Aspect of Physiological Psychology]

V. Movements, pp. 820-858.

Conclusion, pp. 858-863.

It is clear that what I have called the inner aspect of physiological psychology is represented, not at the one combined level of the original schema, but at three successive levels, at each one of which the 'objective' experience has its 'subjective' pair. It is clear, also, that the increased space required for this elaboration has meant the curtailment of Parts III, IV and V of the original outline: movement gets only a scant 40 pages, as against over 600 for its co-ordinate Part; the psychological criticisms are packed away in brief appendices to the successive chapters; and the wide-sweeping Conclusion is reduced to exactly five pages! No doubt, the publisher had his say; Wundt had estimated the size of the work at 640 to 800 pp., and it runs to 872. At all events, the mould was now set. Movement never recovers its lost importance; psychological criticism remains

to the end a matter of appended paragraphs; and the full Conclusion appears only in the fifth and sixth editions.

So far, then, we have the familiar picture of a work which, through wealth of material, has grown under its author's hands until it outruns the appointed limit; the earlier parts are disproportionately long, the later disproportionately curtailed. But I have oversimplified; I have omitted pp. 707-725, the initial section of Part IV. These twenty pages, which are entitled *Bewusstsein und Aufmerksamkeit*, are of extraordinary significance. Their intervention, between the doctrine of idea and the doctrine of association of ideas, means that a second systematic thread, in addition to the thread of sensory integration, appears in all the following exposition.<sup>1</sup> We should expect a reference to it in the first, physiological Part; but there is no apperception-centre in 1873. We should expect, going further back, a reference in the Introduction; but there is no hint there of consciousness and attention. The fact is that Wundt, when he began to write the book, had no notion that he should presently introduce them. It was only when he came to work up the material of his nineteenth chapter, on the course and association of ideas,—his own experiments on reaction and complication, the results of Donders and Vierordt and the rest,—that, as the Preface informs us, he saw the possibility of further systematisation, of a theory of consciousness and attention which should, at any rate provisionally, round off an important division of physiological psychology. The opportunity thus presented was of precisely the sort that Wundt's genius welcomed. He set to work on the new (eighteenth) chapter; he found plenty of cues to back-reference, where there had been none to reference forward; and the doctrine of apperception was incorporated in the *Physiologische Psychologie*. It came in, nevertheless, by way of after-thought; and though it grew to overshadowing predominance,—the stages of that growth are another story,—we may doubt whether it was ever very firmly rooted; whether (to change the figure) it was ever really at home in the general systematic setting of the book.

E. B. T.

#### EXPERIMENTAL PSYCHOLOGY IN ITALY

In his inaugural lecture at the University of Palermo Professor F. U. Saffioti sketches the development of experimental psychology in Italy.<sup>2</sup> The pioneers were three Sicilians, G. Sergi, G. Buccola and S. Corleo. Sergi as early as 1876 advocated the establishment of a laboratory; but nothing came of his efforts before 1889, when a laboratory was founded at Rome as a section of the Institute of Anthropology. Buccola, who died young, worked from 1880 to 1895 with A. Tamburini in the hospital of Reggio Emilia and with E. Morcelli in the hospital and the psychiatric clinic at Turin. Corleo, who died in 1891, started in 1889 a small laboratory, afterwards allowed to lapse, at the University of Palermo. Psychological work, during these early years, was done in hospitals (at Reggio Emilia, for instance, Tamburini and G. C. Ferrari founded a laboratory in 1896) and in physiological institutes (A. Mosso at Turin, M. L. Patrizi

<sup>1</sup> Cf. this JOURNAL, xxxii, 1921, 116 f.

<sup>2</sup> La evoluzione della Psicologia Sperimentale in Italia, *Rivista di Psicologia*, xvi, 1920, 129 ff. Cf. this JOURNAL, xv, 1904, 515 ff; xvi, 1905, 225 ff.

at Modena); lecture-courses at universities appear to have depended altogether on the initiative of the professor (Sergi at Messina in 1878-9, Ferrari at Bologna). The first independent university laboratory was opened in Florence, 1903-4, by F. De Sarlo.

In 1905 the fifth international congress was held at Rome. A direct result was the establishment of three chairs of experimental psychology: S. De Sanctis was put in charge of the laboratory at Rome, F. Kiesow of that at Turin, and C. Colucci received the call to Naples. In 1908 a laboratory of scientific pedagogy at Crevalcore was transformed into a laboratory of pure and applied psychology and, as a communal institute of Milan, placed under the direction of Z. Treves. At the director's death in 1911 this laboratory seems to have declined; his personally owned apparatus were bequeathed to De Sanctis' laboratory at Rome,—where, curiously enough, the psychological section of the Anthropological Institute was still maintained under Sergi's guidance. In 1912 a psychological laboratory, named in honor of Treves, was started by Gonzales and Corberi in connection with the provincial hospital of Milan at Mombello. Finally, Saffiotti was called in 1918 to Palermo, and V. Benussi, apparently in the same year, to Padua.

The *Revista di Psicologia* was started by Ferrari in 1905; it is the organ of the *Società Italiana di Psicologia*, which took shape in 1911. In 1920 appeared the first numbers of the *Archivio Italiano di Psicologia*, edited by F. Kiesow of Turin and A. Gemelli, a pupil of Külpe and Kiesow, now in charge of the laboratory of the Istituto Nazionale Medico-Pedagogico at Milan. Volumes of experimental studies have been issued from Reggio Emilia, Rome, Florence and Turin.

E. B. T.

#### THE PSYCHOPHYSIOLOGY OF THE CONDEMNED

Dr. L. Gualino, director of an Italian war-hospital, has published a paper on the psychophysiological characters of soldiers condemned to be shot for breach of discipline.<sup>1</sup> Pulse is accelerated to 100 at the moment of sentence; is thereafter variable; and sinks to 60 at the place of execution. Sweating is profuse, but a thermal anaesthesia prevents reaction to cold. Salivary secretion is lessened; the voice roughens or fails; tears cannot be shed. Breathing is of the Sikorsky type: the thorax is violently filled or emptied, and the succeeding respiratory movements are superficial and hardly if at all perceptible. There is no impulse to defecate, and no tendency to frequent and scanty urination; a vesical anaesthesia leads, however, to unnoticed overflow of the bladder's contents. The most characteristic physical symptom is a paresis or paralysis of the lower limbs. The face assumes a set, mask-like expression. The pupil alternates between dilatation and normality; in both conditions it reacts normally to light. There is trembling, but (the writer thinks) no true tremor; muscular contractions appear irregularly at various parts of the body, and are probably due to "multiple fibrillary myoclonias." Reflexes are never normal; they may be heightened or diminished. The various modes of general sensitivity show a loss of acuity which may reach actual anaesthesia. The specific sensitivities are rather heightened than impaired.

<sup>1</sup> Psicofisiologia dei fucilandi: Annotazione obiettive, *Rivista di Psicologia*, xvi, 1920, 42 ff. The editor, Professor G. C. Ferrari, adds a brief note (101 ff.) entitled *Psicologia dei moribondi*.

The mental functions, on the other hand, maintain their integrity to the very end. Attention is lively, prompt and well-sustained. Memory is fully efficient. Thought, conception, judgment, reasoning present no change, qualitative or quantitative. The insistent idea is not that of death, or of what is to follow death, or of injustice, but of the unimaginable pain that may attend the moment of dying; the condemned are eager for fact or argument that may reassure them. Nor are the emotions blunted. The principal egoistic emotion displayed, apart from the overshadowing fear, is vanity; the condemned wish to make a good appearance, and are annoyed at the invading paresis; the chief altruistic emotion is a strong and variously directed sympathy. There is little sign of religious emotion. The religion of the condemned, like that of soldiers in general, is strictly utilitarian; their religious images are made, as circumstances suggest, the object of prayer or cursing; they seldom avail themselves voluntarily of the ministrations of the chaplain; and though at last they usually agree to receive the sacraments, this is due more to outside pressure and to the vague notion of a safeguard of the future than to any real religious feeling.

In his conclusion the author points out how far removed from the truth is the common belief that the condemned man is, to all intents, dead before he mounts the scaffold or takes his place on the field of execution. In natural death, it seems that the brain first succumbs, and that spinal paralysis follows later; in these cases of violent death, on the contrary, the cord appears to renounce its functions while the brain remains intact.

E. B. T.

#### LOCOMOTION OF INSECTS

The locomotion of insects when walking with their six legs intact is roughly by three legs at a time, one set of three supporting the body tripod-wise while the other three are advancing. For example, while the right fore leg, the left middle leg and the right hind leg form the tripod of support, the left fore leg, the right middle leg and the left hind leg are advanced; then the last three form the support while the first three are advanced, and so on in alternation. This fact has long been known. Von Buddenbrock set himself to discover what happens when some of the legs (say the middle leg on each side) are removed by accident or amputation, using a walking-stick insect for his experiments.<sup>1</sup> If the original leg-partnerships were to continue, the insect would have a gait like a pacer, moving the fore and hind legs on the same side at the same time. As a matter of fact this is not what happens; but instead it uses the left fore leg with the right hind leg and the right fore leg with the left hind leg after the manner of a trotter, and does so whenever a leg on each side is removed, irrespective of their relation to each other. Removal of a single leg makes no change in the plan of locomotion.

The change of leg-usage when two legs are lacking is advantageous for an insect that must move about back-downward on the underside of leaves and the like, and raises the interesting question how the change is brought about. Is it due to the operation of some general center of locomotor control; is it caused in a purely mechanical way by the changed distribution of the weight of the insect upon the legs

<sup>1</sup> v. Buddenbrock: *Der Rhythmus der Schreitbewegungen der Stabheuschrecke, Dyxippus*; mit 2 Abbildungen. *Biologisches Zentralblatt*, Bd. 41, Nr. 1, Januar 1921, pp. 41-48.



that remain; is it a matter of a local neural mechanism? Von Buddenbrock concludes (on the basis of an extended series of experiments which involved, beside the amputations and ingenious tests of other leg-conditions, the sectioning or hemisectioning of the main ganglionic chain at various levels) that the last suggestion is the most probable. The change appears to be in reality no change of locomotor plan at all, but merely the operation of the original neural mechanism within which the relative excitabilities have been altered by the amputations.

For details the reader will wish to consult the original. It may be interesting, however, to note the following additional points with reference to the probable distribution of function within the nervous system of this insect. The *regulation* of the movements of locomotion seems to depend upon the three pairs of thoracic ganglia, but its *inception* upon the suboesophageal ganglion; the beheaded insect remains standing stock still. The head-ganglion or "brain," the receiving center for excitations from the organs of special sense, operates *inhibitively* for the most part, causing normally a quasi-cataleptic rigidity characteristic of the adult insect during the daytime; while removal of the "brain" results in almost continuous locomotor activity.

E. C. S.

#### GEORGE TRUMBULL LADD

Professor Ladd was born at Painesville, Ohio, January 19, 1842, and died at New Haven on August 8 of the present year. He graduated from Western Reserve College in 1864, and from Andover Theological Seminary in 1869. From 1869 to 1879 he filled pastorates at Edinburg, Ohio, and Milwaukee, Wis.; in 1879 he was appointed professor of philosophy at Bowdoin College, and in 1881 he was called to Yale, where he remained in active service till 1906. He was a lecturer at Andover Theological Seminary in 1879-81; conducted a graduate seminary at Harvard in 1895-96; lectured in Japan in 1892 and 1899, and in India in 1899-1900; and was president of the American Psychological Association in 1893. His books, on theology, philosophy, and psychology, make a long list: his chief psychological works are *Elements of Physiological Psychology*, 1887 (second edition, in collaboration with R. S. Woodworth, 1911); *Philosophy of Mind*, 1891; *Primer of Psychology*, 1894; *Psychology, Descriptive and Explanatory*, 1894.

Ladd was not, in my judgment, a great psychologist; but he was competent and assiduous; and his publications were of very distinct service to the cause of psychology in this country. His *Physiological Psychology*, in particular,—I well remember my excitement on finding this book in the library of the Oxford Union, and the shock of disappointment at reading that mind was a real unit-being!—helped toward the establishment of laboratories and the recognition of experimental psychology as an academic study. Coming, as it did, from a professor of philosophy at Yale who had been a Congregational minister, it gave the young science an air of respectability (I can think of no better word) which was of high advantage in its struggle for life. Aside from this special value, however, the book was important as our one English text-book and book of reference. Those were primitive times; James' *Principles* were still three years away; the translation of Ziehen's little *Physiological Psychology* appeared in a first edition in 1892; and the first instalment of Sanford's *Course*

came out in 1894. Even in Germany Wundt's *Physiologische Psychologie* was only just attaining its third edition, and Münsterberg's *Willenshandlung* had not yet seen the light. Ladd had no model save the Wundt of 1880, and his volume embodies an amount of hard work for which we owe him grateful thanks.

By a coincidence which I wish might have been avoided this number of the JOURNAL contains a detailed criticism of Ladd's systematic psychology. I had looked forward to a trenchant rejoinder.

E. B. T.

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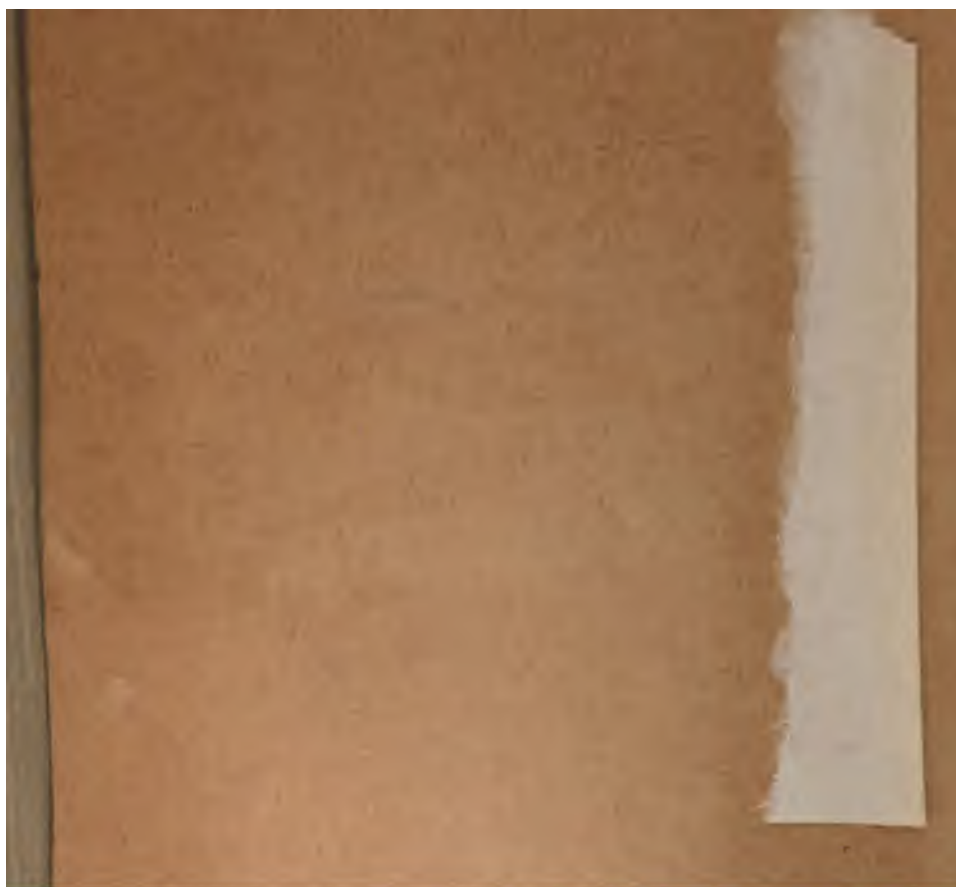
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